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EDITED BY

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INTRODUCTORY NOTE.

With the present volume we do not so much desire to augment the number of psychological periodicals as to inaugurate an exponent of psychological research in Canada, which has hitherto been without a representative publication. The Psychological Series of the University of Toronto Studies appears at irregular intervals and is intended to contain for the most part the results of research in the Psychological Laboratory of the University of Toronto, although contributions from elsewhere may be accepted. The Laboratory, founded by Professor J. Mark Baldwin in 1891 (and described by him in its then state in the March number of *Science*, Vol. XIX., 1892, page 143, etc.), has considerably expanded in recent years. It is therefore expedient for the information of the reader in regard to the localities referred to in our articles to append to this volume a small plan of the Laboratory in its present condition.

A. Kirschmann.

University of Toronto, Nov., 1900.

INDEX TO THE ACCOMPANYING PLAN

1. Small Lecture room, at present used also for optical research.

2. Room for class-work, also used for experiments on reaction-times, etc.

3. Private room of the Director.

4. Dark room (for photographic purposes).

5. Larger Lecture room.*

6. Store room for demonstration apparatus.

7. Library and reading-room.

8. Assistants' room.

9. Research room for psychological optics.

10. " "

11. Dark room (for shadow experiments, photometry, etc.)

12. Research room.

13. " (illusions of weight at present).

14. Ethnological Museum, temporarily used by the Psychological Department for research in colour-æsthetics (skylight).

15. Acoustical room.

16 (with two annexes). Research room for optical experiments (skylight).

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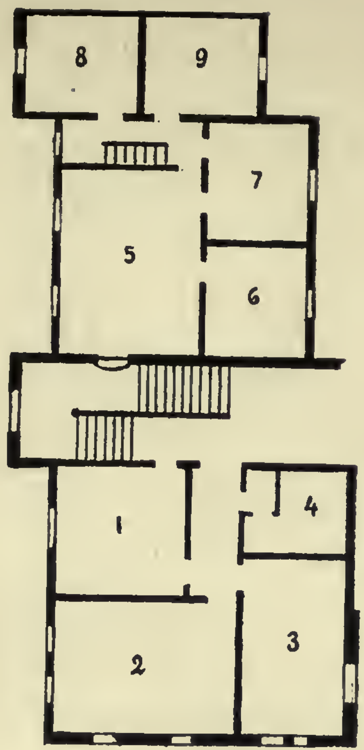
* The Lecture rooms used for the larger classes are not included in this plan.



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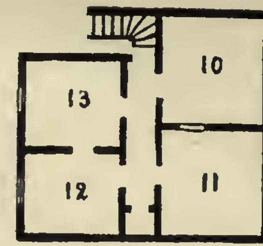

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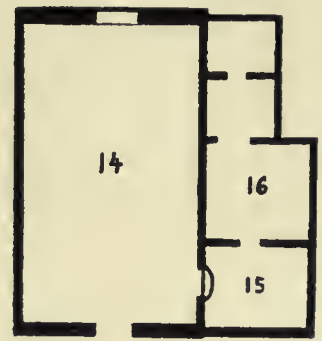

SECOND FLOOR

PLAN OF THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF TORONTO.

SCALE 1 : 300.

TABLE OF CONTENTS

	Page.
The Space Threshold of Colours and its dependence upon Contrast, by W. B. LANE, M. A	1
A Case of Abnormal Colour Sense, examined with Special Reference to the Space Threshold of Colours, by J. W. BAIRD, B.A., and R. J. RICHARDSON, B.A	86
Additional remarks on Colour Blindness, by A. KIRSCHMANN, Ph.D	97
A Contribution to the Psychology of Time, by M. A. SHAW, B.A., and F. S. WRINCH, M.A	105
Experiments on Time Relations of Poetical Metres, by A. S. HURST, B.A and JOHN MCKAY, B.A	155
Conceptions and Laws in Aesthetic, by A. KIRSCHMANN, Ph.D	177
Experiments on the Aesthetic of Light and Colour: On Combinations of Two Colours, by EMMA S. BAKER, B.A	201
Experiments with School Children on Colour Combinations, by W. J. DOBBIE, M.A	251

INDEX OF ILLUSTRATIONS

To face page

1. Ground plan of the Psychological Laboratory of the University of Toronto iv
2. Apparatus for the investigation of the Space Threshold of Light and Colour Sensations, Fig. 1 and 2 22
3. Photograph of the inverted spectrum, Fig. 1, 2 and 3 100
4. Apparatus used for experiments on the estimation of time-intervals, with tracing, Fig. 1, 2 and 3 128
5. Graphical representation of the æsthetic value of combinations of two colours, plates 1 to 8 (curves I to XXIV) 228
6. Combination-curves, plates 9 to 15 (curves XXV to XXXVII) 232
7. Diagram representing the complementary relations of the Prang standard colours (plate 16) 242

18/24

THE SPACE-THRESHOLD OF COLOURS AND ITS DEPENDENCE UPON CONTRAST

FIRST ARTICLE

By

W. B. LANE, M.A.

THE SPACE-THRESHOLD OF COLOURS AND ITS DEPENDENCE UPON CONTRAST.

FIRST ARTICLE

BY W. B. LANE, M.A.

I.

INTRODUCTORY.

In an article by Mr. J. O. Quantz, B.A., published in the American Journal of Psychology, Vol. vii., No. 1, the dependence of size estimation upon colour is discussed. This problem naturally suggested a converse inquiry into the dependence of colour perception upon size. But colours are not ordinarily seen in complete isolation. There is always an environment which exercises, through contrast, a decisive influence upon the perception of colour. It therefore appeared desirable not merely to investigate the question under conditions of contrast, which in some form was unavoidable, but to examine into the specific nature of the relation between particular kinds of contrast, *e.g.*, pure colour contrast and magnitude of coloured surface.

The field of optical contrast is of great extent and includes a considerable variety of distinguishable influences. It will be necessary to discriminate between them both for the purpose of investigating the bearing of contrast upon the problem in hand, and in order to define the particular kind of contrast with which we are concerned. Optical contrast has been divided into successive and simultaneous contrast. The former, successive contrast, is identical with the phenomenon of after-images. If one turns away after looking at a dark surface on a bright ground, one will have a series of alternating positive and negative after-images, in the latter of which the ground will appear dark and the dark surface bright. This inversion of the relationship between surface and ground in the negative after-image is a case of so-called successive contrast. That part of the object of consciousness which at one moment was bright appears at a subsequent moment dark, and the dark portion appears bright, a fact which seems to indicate that the relationship between the parts of the after-images in respect to brightness is inversely dependent upon the same relationship as it exists between the parts of the preceding original images. Helmholtz in his *Physiological Optics* states the law thus: "After looking at a colour *A* of moderate intensity then look at another *B*. If the after-impression is not sufficient to produce a positive image it produces a negative image of *A* on *B*. The parts of *B* which are in the same place as *A* are dimmed. If *A* and *B* are of the same tone then *B* is rendered more whitish, if they are complementary *A*'s saturation increases. If *B* is between *A* and its complementary it passes for a neighbouring tone which is further from *A* and nearer its complementary. Otherwise *B* becomes dark as much as *A* becomes more bright." It was from this kind of contrast that we sought to free our experiments as far as possible.

Simultaneous optical contrast embraces all, and more than all the phenomena of contrast whose connection with the space threshold of colour I seek to trace in this paper. It includes all such phenomena as the modification in apparent size, colour, etc., which one visible surface exerts upon our visual impression of another surface at the moment of that impression. There may be several kinds of modifying influence by which to classify the facts of simultaneous contrast. It may consist in (*a*) modification of the apparent size of the surface (extension contrast), (*b*) modification of the apparent brightness of the surface (brightness or intensity contrast), (*c*) modification of the saturation in the case of a coloured surface (saturation contrast), (*d*) modification of the colour tone of the surface (colour contrast), or (*e*) modification in the emotional tone accompanying the perception of the surfaces.* In our usual experience these kinds of contrast appear for the most part in combination, but they are for all that quite distinct, and, in scientific abstraction, separable features of ordinary contrast effects. The only one likely to be obscure is saturation contrast, to which alone therefore I will briefly refer. By saturation as distinct from colour tone and brightness is meant the degree of colour quality as compared with the absence of the same. By colour tone, on the other hand, is meant the degree of transition in a closed manifoldness of

* For a discrimination of the various kinds of simultaneous optical contrast see American Journal of Psychology, Vol. iv., No. 4: "Some Effects of Contrast," by Dr. A. Kirschmann, and the same author's inaugural dissertation, "Ueber die quantitativen Verhältnisse des simultanen Helligkeits- und Farben-Contrastes," in Philosophische Studien, Bd. vi., p. 417.

colour wherein the tone is determined, not by relation to the absence of colour quality, but by relation to other colour qualities of the same manifoldness. For example, green, red, orange, etc., are transitions in a closed series of colour which begins and ends in colour, but between each of these, taken separately, and the disappearance of colour altogether there is a series of transitions which rise from the zero point of colour quality to the highest possible degree of it. These latter transitions are called degrees of saturation of the particular colour, whether it be green, red, orange, or any one of the infinite possible transitions in the closed colour manifold. In other words if we represent the transitions of colour tones graphically as a circle we must convert our two-dimensional circle into a geometrical figure of three dimensions in order to make it also represent the manifold of possible saturations. That saturation, and therefore saturation contrast, are not mere matters of speculative formula, which can practically be neglected in considering the possible contingent influences to be eliminated in an exact examination of colour contrast, is shown by the fact that it is possible to vary the saturation of coloured surfaces without changing the light intensity or colour tone. Dr. Kirschmann has succeeded in doing so by means of colour discs. For a description of the means employed I refer to his article "Colour Saturation and its Quantitative Relations" (American Journal of Psychology, Vol. vii., No. 3), in which he has given a preliminary account of the subject. For my purposes it suffices to point out that saturation contrast, which means contrast between different degrees of saturation, all of equal light intensity and the same colour, is a possible contingency which had to be taken account of in the prosecution of our contrast study.

Since in any ordinary case of colour contrast there are these different influences at work, it was necessary to disentangle them as far as possible and to confine our investigation to one alone. Accordingly in our inquiry we sought to limit ourselves mainly, if not absolutely, to colour contrast, by which is meant contrast of colour tone, not in the ordinary careless use of the term but in its strict significance as distinguished from saturation and intensity. Our problem then in its bearing on contrast phenomena is mainly to examine the relation which holds between colour contrast and the magnitude of coloured surface in the initial stages of the perception of a coloured object. By magnitude is meant not the absolute size of the surface but the visual size, if we may so speak, that is, the size as relative to the perceiving eye, which the absolute size of the surface and its distance from the eye conjointly determine. The visual size of an object is measured by the angle which that object subtends at the centre of the pupil of the eye, an angle which is named the visual angle and which for any object of constant magnitude varies with the distance from the eye. Of course for every object at any distance there are as many visual angles as it has different diagonals and diameters. If the visual angle (selecting for the purpose either a diagonal or a diameter angle) at which a coloured surface is first distinguishable in its proper colour tone may be called the spatial threshold of that colour, we define our present problem as an inquiry into the spatial threshold of colours, especially when those colours are subjected to the contrast influence of other colours. Incidental to the examination of this problem will come naturally also an investigation into the influence of colour contrast upon two other possible spatial thresholds, viz., (1) the threshold at which a coloured surface appears first as light, and (2) the threshold at which such a surface appears coloured though not in its proper colour tone. These will form subordinate problems whose elucidation will necessarily take place concurrently with that of the main problem. For it has long been well known that a colour, whether under the contrast influence of other colours or not, does not retain its characteristic colour quality at very small visual angles, nor does it, by the gradual enlargement of the visual angle from zero, emerge at first upon the vision fully formed in its proper tone. On the contrary almost all colours lose their special colour quality at determinable small visual angles and appear either only as light or as some other colour. A very familiar example is the greyish appearance of a distant autumnal hillside, clad in leaves which are beginning to change their tints. At a distance of three or four miles it is not only impossible to distinguish the colour qualities of the variously tinted surfaces, which on closer inspection are seen to be massed into a mosaic of peculiar beauty, but their joint effect at a distance is frequently that of a characterless russet grey. The reason is that each of the colour surfaces subtends so small a visual angle that its stimulus is below the threshold of perceptibility for that colour; and while at nearer view they would make good to the spectator their many differences of colouring, yet in the greater distance the purple, orange, yellow, red, green, brown, with their many shades and hues to be found in such a landscape, are indistinguishably blended in one poorly saturated reddish colour, or even reduced to a plain colourless grey. Another illustration of the same fact is our inability to distinguish the violet colour of a pansy at a distance which is not great enough to render the object itself quite invisible. On withdrawal of the spectator to sufficient distance the flower becomes indistinguishable from a dark grey or black spot and at still greater distance disappears entirely. When we approach again it gradually becomes visible, first as a point of grey light and then as coloured though not yet violet. Still nearer the violet character of its colouring is distinctly perceptible. From these familiar examples the fact is illustrated that most coloured objects have three different stages of perceptibility which are dependent upon their spatial relations to the eye of the percipient. The first is when they are merely visible but without colour at all; the second when they are seen to be possessed of colour though not the proper colour; and the third when they become visible in their correct colour quality. The exact delimitation so far as possible of these three thresholds of colourless, chromatic and characteristically coloured light for all the variety of colour presented by the spectrum is the aim of the present paper. The problem with which we started was to determine the last of the above, but in dealing with it the question of the two former naturally arose. In the perception of a coloured object the lower thresholds must be passed through before the third is reached, and therefore they also properly fall within the scope of this paper and were investigated at the same time as the main problem.

I do not mean to imply that every colour will have three thresholds absolutely distinct from one another without the possibility of a coincidence of any two of them or of all three. There are many facts apart from those which have made their appearance in the course of our experimental treatment of the problem which would bear strong evidence against such an assumption. It has perhaps frequently been noticed that a red light, for instance the port light of a vessel at sea or the danger light of a train, remains visible as red almost or quite as long as it is visible at all, which goes to indicate that its achromatic, chromatic and characteristic colour thresholds as distinguished above are not separated but coincident. Our experiments have corroborated such observations, showing in a more exact and quantitative way that red is a colour which does not easily or at smallest surface sizes lose its colour quality, but is for the most part distinguishable as red just as long as it can be discerned at all in the diminution of its extension. For most colours, however, it will be found that the three stages of discernment are distinct, and consequently will require separate examination.

II.

HISTORY OF THE PROBLEM.

The phenomenon of a space threshold of colour was noticed in the early history of physiological and psychological science although it did not receive careful investigation. It was noted and passed by without any thorough inquiry into the details or quantitative relations of the problem. Such cursory treatment practically amounted to nothing more than calling attention to the existence of a subject that demanded scientific investigation. Within recent years, however, some attempts at explanation have been made which yet, as we shall see, have not completely satisfied the conditions of such an inquiry.

As early as 1823, the dependence of perception of colour upon the visual angle of the coloured surface was noticed by Purkinje.* He called attention to the fact that both intensity and visual angle play an important rôle in the perception of colour. " Sensibilitas oculi in specificam coloris cuiusdam qualitatem ad diversas distantias et sub certis gradibus luminis examinari poterit, nam notum est qualitatem illam colorum in objectis affatim minutis ad justas distantias evanescere."†

Plateau was the first, perhaps, who noticed and recorded the fact that colours disappear as colour at very small visual angles of the coloured surface.‡ However, he made no further use of his observation, and did not attempt any detailed inquiry into the subject. In fact none of the early investigators sought to determine with any exactitude the nature of the relationship which they observed between the visual size of a coloured object and its perceptibility as colour, and, so far as I can find, no fruitful advantage was taken of the fact which these early scientists had noted. Not until comparatively recent times, by Von Wittich and Aubert, was any attempt made to determine quantitatively the relation of colour perception to the size of the coloured surface.

* Commentatio de examine organi visus, etc. Breslau, 1823. (P. 15.)

† Quoted by Aubert in his *Physiologische Optik.*

‡ Poggendorff's Annalen, Bd. 20, 1830. (P. 327.)

Von Wittich records in the Königsberg medical Year Book some interesting experiments made to ascertain what were the smallest visual angles at which the several colours could be perceived, either at all and in any form, or as of their proper quality. The results obtained were twofold, the objects being viewed momentarily on the one hand and on the other continuously for sufficient time to render possible a secure judgment. The following tables give his results which will subsequently be used for comparison.

Black Ground.

Colour	Momentary view				Continuous view			
	Visible		Coloured		Visible		Coloured	
	′	″	′	″	′	″	′	″
Red	1	23	1	58	1	4	1	4
Orange	1	4	1	32	1	4	1	4
Yellow orange	1	9	1	32	1	14	1	14
Yellow	1	23	1	23	1	4	1	4
Pure green	1	23	1	43	1	4	1	43
Dark green	2	17	6	53	2	17	3	16
Pure blue	1	14	2	17	1	14	1	43
Dark blue	2	17	7	38	1	58	3	26
Rose	1	14	2	17	1	32	1	32
Violet	1	43	6	53	3	26	3	26

White Ground.

Colour	Momentary view				Continuous view			
	Visible		Coloured		Visible		Coloured	
	′	″	′	″	′	″	′	″
Red	1	23	6	53	1	9	2	50
Orange	1	43	3	8	1	4	1	32
Yellow orange	2	17	5	43	2	17	2	50
Yellow	6	53	6	53	3	26	3	26
Pure green	2	17	3	26	1	43	1	43
Dark green	1	23	13	46	13	46	13	46
Pure blue	1	23	4	35	1	9	1 (2	43 17)
Dark blue	1	23	5	17	1	4	2	17
Rose	1	43	4	35	1	9	2	50
Violet	1	32	13	46	1	9	5	43

As Plateau had previously seen, when he said that coloured objects at small visual angles appear as a scarcely perceptible cloud, Von Wittich noticed that colours at very small visual angles lose their characteristic tone and appear either as quite colourless light or of some other colour tone. In his experiments tabulated above he therefore investigated what might be called two space-thresholds of colour perceptibility, the one where the surface is barely visible at all and in no case correctly coloured, the other where it is seen coloured. In short he has treated what we have already distinguished as the achromatic space-threshold and the chromatic space-threshold, though he has not made the distinction between the merely chromatic and the proper colour threshold (or what we have designated the characteristic colour threshold), wherein the coloured surface is not only seen to be coloured but, what is quite different in many cases, seen coloured as it actually is.

The means which Von Wittich employed in his investigations were small coloured squares which he moved to and from the observer by gradual transition until the different threshold points were reached. The distances from the observer and the absolute size of the coloured objects being known, it was a matter of easy calculation to interpret the observations in the form of visual angles.

Von Wittich in his work noticed and emphasized a fact which was disclosed also in the course of our experiments, that when an object becomes barely visible it becomes so only momentarily and will appear and disappear again in the most capricious manner. Especially is this the case in a darkened room where the observer has no distracting light to employ his attention except that which is looked for from the object. Then the point of light will break into the field of vision from the most unexpected quarter, will dwell for scarcely a moment, then suddenly disappear to appear again in the vision field in some other place. Von Wittich also noticed the same peculiarity of momentary visibility in the emergence of the colour sensation. He has recorded the observation that in the first stages of colour perception, the colour, as before in the case of mere light, is seen only intermittently, and that too only in the case of movement either of the eyes or of the object itself. He gives a very interesting example of this necessity of movement in his experiment with coloured fibres, some of which were fixed at a certain distance and some were in movement. The moving fibres were seen as coloured from a greater distance than those at rest.

thus indicating that movement certainly plays an important rôle in the perceptibility of colours. We have found frequent confirmation of this observation of Von Wittich's in the course of our experiments. It was a common experience for our observers, after having affirmed that they saw the light point coloured, to recall the affirmation because the colour had instantly changed again into the point of colourless light. And the reason was not far to seek. In their endeavour to identify the colour just emerging they had fastened their eyes upon it too fixedly, with the consequence that the colour quickly disappeared. As soon as the fixedness of their gaze relaxed and the eye was permitted to perform those slight involuntary movements, which always accompany a clear visual perception, the colour reappeared forthwith.

Von Wittich did not fail to notice the fact that a considerable difference in the space-thresholds of colours obtains, according as those colours are viewed upon a black or upon a white ground. An examination of his tabulated results given above shows that the colours he employed have usually somewhat higher space-thresholds against a white than against a black ground. There are some notable exceptions, as, for example, dark blue, which on a white ground by momentary inspection is first visible at a visual angle of 1′ 23″, and first seen as coloured at a visual angle of 5′ 17″; on continuous view it is first visible at a visual angle of 1′ 4″, and first seen coloured at 2′ 17″. On a black ground momentarily viewed it is first visible at 2′ 17″ and coloured at 7′ 38″, steadily viewed it is first visible at 1′ 58″ and coloured at 3′ 26″. There are other incomplete exceptions such as violet which on a white ground is seen uncoloured earlier than on a dark ground, although the discernment of it as coloured comes at a very much greater visual angle for white ground than for dark. The probable explanation of these exceptions is to be found, it seems to me, in the fact that the black which Von Wittich employed was black cardboard, by no means a good specimen of non-reflecting surface. As compared with the black of a good velvet, black cardboard is grey. This goes to show that surfaces which we call black are only relatively black and consequently that a haphazard choice of them for employment as black grounds in experimental work might easily lead to a condition where the ground actually reflected scarcely less light than some of the colour pigments. It is likely that something of the kind has been the case in Von Wittich's experiments. The violet and blue pigments which he used were pigments which relatively to the other colours reflected very little light and consequently were much more akin to black than the rest. The contrast of brightness intensities between ground and pigment in the case of these two would be much less when the ground was black cardboard than when it was white, whereas with the brighter colours the exact opposite would probably occur. We should therefore naturally expect that violet and blue would prove themselves at variance with the other pigments in regard to their colour thresholds on white and black grounds, that they would in fact have lower thresholds on the white ground than on the black, while the others had lower thresholds on the black ground. On the other hand, had the black ground used been not merely a comparative black, such as cardboard or even velvet, but actually a surface reflecting no light whatever, then it is possible that the blue and violet would have followed the same rule as the other pigments.

Aubert, in the work which he has done, so far as it bears upon the problem of the space threshold of colours has largely followed in the footsteps of Von Wittich. He has indeed refined somewhat upon the means employed but in the total outcome he has made very little advance. He employed coloured squares of two millimetres in diameter placed equidistantly from the spectator in diffuse but clear daylight. The observer withdrew to a distance at which the squares could no more be seen. He then gradually approached to such positions as would respectively enable him just to see the objects as uncoloured, advanced further until he could see them as coloured, whether properly or not, and still further until he saw them in their proper tones of colour.* By measuring the distances of the observer's several successive positions he had the material for a calculation of the visual angles which coloured square surfaces must subtend in order to be, first, seen at all, secondly, seen coloured and thirdly, seen coloured correctly. In order to rest the vision Aubert also used in his experiments darkened tubes for the eyes with a mask to close off the diffuse daylight which illuminated the colour pigments.†

The results which Aubert attained were considerably different from those of Von Wittich. This difference he ascribes to three

*Abhandlungen der schlesischen Gesellschaft (Breslau, 1861), and Physiologische Optik.

†Aubert, Physiologie der Netzhaut, p. 115.

main causes, (1) the place of the illumination, (2) the pigments used and (3) the subjective uncertainty concerning the extreme limits which can be set to perceptibility. He found* that orange appears coloured from the first, at 39″ as red, at 59″ as orange; red on a black ground at 59″ is seen as red, while on a white ground at 59″ it is dark, almost black, and becomes coloured at 1′ 43″; ultramarine blue on a black ground at 1′ 14″ is grey, at 4′ 17″ blue, while on a white ground at 1′ 8″ it is black and only becomes visible as blue at 5′ 43″; bright blue and bright green appear at 1′ 8″ both equally grey (light grey on black and dark grey on white ground), while they become discernible as blue and green at about 2′. The following further results are taken from some notes on Aubert's *Physiologie der Netzhaut*:—Rose on a black ground appears grey at 39″, at 59″ yellow, at 1′ 8″ golden yellow, at 1′ 23″ reddish yellow, at 3′ 47″ rose; dark brown at 1′ 8″ is seen as fawn-coloured; green at 1′ 8″ is bluish at 1′ 49″ green; on a white ground bright blue is seen as black at 1′ 8″ and at 1′ 49″ as dark blue. Orange is first seen coloured at 35″; red is first seen coloured at 39″, green at 44″, blue at 2′ 7″, yellow at 41″.

From the above figures it will be plainly seen that the discrepancies which Aubert himself noticed between his own and Von Wittich's results and between his own at different times are not inconsiderable, so far at least as the absolute numerical values of the visual angles are concerned. Whether the relative values among the various colours show any similarity or regularity as between the results of the two investigators we are unable to pronounce upon, because the colour list which Aubert used and that of Von Wittich have few points in common. Had they both employed the same set of colours it might have been shown that although there were considerable discrepancies in the actual figures given by the two for any one colour, yet the mutual relationships among the various colours were fairly constant.

Aubert has made one material advance upon Von Wittich's work in the investigation of this problem. He has contributed to the more definite distinction of the mere chromatic from the characteristic space threshold. Von Wittich combined the two in the records of his observations although he did not fail to notice the existence of the distinction. Aubert not only definitely enunciates the distinction, but also gives it a quantitative expression and

*Aubert, Physiologische Optik, p. 537.

notes the visual angles of the colours quite as carefully when they are first seen coloured, though incorrectly coloured, as when they are seen in their proper tone.

The work which Von Wittich and Aubert have done upon this problem of the space-thresholds of colour perceptibility cannot but be seen to be incomplete in method and results. Its deficiencies, however, are largely those which attend pioneer efforts in any line of work. For them the question arose as a curious fact of no small interest in the midst of other problems more immediately engrossing, with the inevitable consequence that the investigation was not completely carried out in all its various and obvious divisions. The subject which they dealt with in a somewhat incomplete way is capable of considerable extension. They recognized in some degree the influence which contrasting surfaces have upon the perceptibility of colours. Thus they took into account the difference of white and black surfaces in this regard, examining their colours when subject to the contrasting influence of black and white grounds. But although it is quite as frequent in every-day experience for colours to be seen upon backgrounds of other colours as upon the colourless grounds of white and black, yet it never seems to have occurred to these early investigators to examine the perceptibility of colours under the ordinary influences of colour contrast. It is not improbable that colour contrast would introduce quite as considerable variations into their results as did the contrast with white and black. And indeed our own experiments have confirmed this *primâ facie* probability as will be seen subsequently in the exposition of our results.

Apart from the omission of Von Wittich and Aubert to extend their investigation to the full limits of the problem we cannot fail to notice the unsatisfactoriness of their method even within the circumscribed sphere of their inquiry. What precautions did they adopt to insure with fair reliability that the thresholds which they found for the perception of colour were really assignable to the colour perception *per se* rather than to colour perception aided or retarded by some other accessory conditions? For example, can we be sure that the threshold of 39″, at which Aubert finds that orange is seen as red, is really the chromatic threshold of orange, unaffected by contrast of light? It appears to be quite probable that this result is in part due to the influence of the contrasting intensities of the colour pigment used and its surroundings. It is indeed true that these investigators do not attempt to maintain that their results indicate anything but the perceptibility of colours when seen upon certain specific grounds, namely black and white. Such results may have served their purpose and observations on that basis may be of considerable practical value. But I must insist that the mere fact of their performing their experiments under certain conditions of contrast is far from being a recognition or discovery of the exact part which contrast plays in the matter. Again it appears to me to be scientifically desirable to remove in some way if possible the contribution which intensity contrast brings to the results recorded, and to isolate the conditions that produce pure colour perception. Neither Von Wittich nor Aubert made any attempt to separate the factors that entered into the colour perceptions which they examined. I do not mean to suggest that it would be possible to remove the factor of light intensity altogether from the field in order to get at the perception of colour pure and simple. This would be to eliminate colour itself, because every colour must have a certain intensity as light. But it is not necessary to do this in order to secure the separation of the two factors. What is necessary is to prevent inequality of light intensities and thus to remove the disturbing influence of light contrast. This would be effected if it were possible to equalize the intensities of ground and colour.

It is very likely that neglect to eliminate the influence of light contrast between the pigment surfaces examined and the grounds on which they were seen contributed largely to the disagreement which Aubert finds between his own results and those of Von Wittich and also to the discrepancies which occur in his own observations; not that they are altogether due or even mainly due to this cause, but such an influence must indisputably have had a great effect in modifying results, and in the nature of the case must have been brought into play in different degrees not only in Von Wittich's experiments, as compared with Aubert's, but likewise in the various examinations of each pigment made by either of them. This is plain from the fact that scarcely any two pigment surfaces have exactly the same light-reflecting power. And the same is true of the black and white surfaces. It can be photometrically shown that some surfaces which we call black reflect many times more light than others which we likewise call black. The terms black and white are in fact elastic in their significance, each including an indefinite number of surfaces of varying degrees of light-reflecting power.* Since such is the case it becomes plain that the quantitative relation between ground and colour in respect of light intensity must vary not only according to the blacks and whites used for grounds but also according to the several colours examined; and not only will this variation depend upon the essential differences between the pigment colours, such as green, red, etc., in light-reflecting power, but it will exist no less concomitantly with the difference in the pigment paper employed for each colour by two or more experimenters, or by any one of them at different times Accordingly, the observations of Von Wittich and Aubert may be expected to show greater discrepancies for some colours than for others, and for some tones of the same colour than for other tones. In this manner the threshold differences among the various colours would be exaggerated. Moreover the natural divergencies for the same colours which arise from varying individual sensibility among observers would be accentuated by the use of slightly different pigments, which would render them unduly and in indeterminable degrees greater or less than if exactly the same conditions of light-intensity-contrast obtained. It is therefore apparent that from this uncertain variable, arising from contrast of light intensities not being converted into a contrast by equal distribution to all colours, a means was afforded for the entrance of considerable discrepancies into the results.

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*Dr. Kirschmann has made interesting experiments which illustrate this statement very well. He records them in his article "Ein photometrisches Apparat zu psychophysischen Zwecken" published in vol. v. of Wundt's Philosophische Studien. He compares photometrically various blacks with a standard white—

I. Paris Black:
- (1) in lamplight (petroleum) $\frac{1}{59.9}$ of the intensity of white.
- (2) in gaslight $\frac{1}{58.2}$ of the intensity of white.
- (3) in diffuse daylight $\frac{1}{57.2}$ of the intensity of white.

II. China Ink:
- (1) lamplight $\frac{1}{23.6}$ of white.
- (2) diffuse daylight $\frac{1}{20.2}$ of white.

III. Graphite (Faber BB):
- (1) lamplight $\frac{1}{5.6}$ of white.
- (2) diffuse daylight $\frac{1}{5.9}$ of white.

IV. Graphite (Faber B):
- (1) lamplight $\frac{1}{5.2}$ of white,
- (2) diffuse daylight $\frac{1}{5.2}$ of white.

This goes to show the photometric variability in what are ordinarily accepted as good blacks—a range broadly speaking from $\frac{1}{60}$ to $\frac{1}{5}$ of the white chosen for standard of comparison.

From another standpoint their method of investigation appears to be unsatisfactory and to give results of merely individual validity. It seems that both Aubert and Von Wittich employed ordinary pigment papers, without making any attempt at controlling or checking some other obvious errors thereby introduced. For such pigments when examined under the spectroscope are found to contain not only the particular kind of coloured light under the name of which each passes, but also in varying degrees many other spectral colours. The blue which we see in a blue pigment paper is for the most part merely a predominant element in a mixture of many similar colour elements. It is not necessary even that there should be any actual rays of the specific kind which gives its name to the pigment paper. Some violet pigments do not emit any violet rays whatsoever, but only blue and red. Frequently a pigment gets its colour not because of the predominant presence of rays of that colour but because of the weakening or absence of its complementary in the assemblage of colour elements. This is chiefly the case with the best yellows, which derive their specific quality largely from the attenuation of the blue rays. It is plain that unless some means are employed to diminish these foreign colour elements so as to reach a comparatively pure colour, we are not sure that the results obtained by the use of these mixed colours are attributable to one rather than to several co-operating colour elements. In fact the conclusions of Von Wittich and Aubert are entirely inapplicable to pure colours and only held good of specific adulterated specimens, the colour pigments which they chose to operate upon. This fact, as Aubert has indicated, would account partly for some of the differences which occurred between his results and those of Von Wittich, for the pigments they used as representative of the various colours would differ in the degree of their colour saturation and in freedom from admixture of foreign elements.

It is a conspicuous deficiency in Aubert's treatment of this problem that he picks up colours at haphazard and in a popular way, without any attempt to make his examination cover the complete field. We find him, as well as Von Wittich, including in his list such specimens of unscientific commercialism as brown or fawn and omitting from it such important colours as violet (which Von Wittich, however, includes) and purple. Brown and fawn-colour so far as they are colours at all, are nothing but more or less liberal saturations of the standard colours. Some browns are merely reds of comparatively poor saturation and light intensity; others are yellows. An interesting experiment is recorded by Dr. Kirschmann* which brings out this fact. It consisted in endeavouring to detect brown surfaces by looking through tubes blackened on the inside and of aperture amounting to about one square inch. In each case browns were judged to be red or orange or yellow, and thus it was shown that they are not simple colour tones, such as red, but that they obtain their peculiar colour characteristic for our vision by such accessory influences as contrast.

We have made some experiments under very much the same conditions as Aubert and Von Wittich, namely, without taking care to eliminate intensity contrast, and using ordinary pigments with daylight illumination. Our sole object was to institute a comparison with Aubert's and Von Wittich's figures. The accompanying table contains the results of our observations when the colours were seen on a black ground. The angular values are calculated on the basis of the diameter (not the diagonal) of the square opening in the brass diaphragm, since apparently Aubert's and Von Wittich's calculations were made upon the the same basis. In our other work, for greater convenience in the adjustment of our apparatus, we have adopted the diagonal basis.

Colours.	Seen first as light.		Seen first coloured (chromatic threshold).		Seen correctly coloured.	
	′	″	′	″	′	″
Purple		52.98	1	30.3	1	58.66
Red		55.56		55.56		55.56
Orange		48.64		48.64	3	56.78
Yellow	1	14	2	39.52	2	39.52
Yellow-green		42.84	2	39.44	2	58.86
Green		46.9	2	10.48	4	2.56
Blue-green		48.4	2	9.88	6	33.14
Blue		53.04	3	15.7	4	30.48
Violet		58.12	4	3.58	5	41.76
Grey	1	17	6	19.72	....	

It will be noted, as was to be expected, that our results differ considerably from those of either Von Wittich or Aubert. It would indeed have been strange if they had coincided in all points because

* "Colour Saturation and its Quantitative Relations," American Journal of Psychology, Vol. vii., p. 391 (1896).

no precautions were used to rule out the many modifying accessory influences which we have spoken of above, except perhaps that care was taken to secure a very close approximation to an absolute black ground and thus to eliminate in part one variable in the undetermined disturbance introduced by intensity contrast. The observation tube was continued to the brass diaphragm by means of a pasteboard annex, which fitted tightly, without any crevices to admit light either at the point of junction with the observation tube or where it joined the plane on which the diaphragm was fastened. The necessary openings to allow of free movement of the sliding plate of the diaphragm were completely obscured by pendent velvets attached to the annex tube. The annex was lined with black velvet and the surface of the brass diaphragm was painted black. By these means was secured a very good specimen of black upon the ground within which the colours were seen.

In the main our results show a lower achromatic threshold than Von Wittich's, and even than Aubert's with a few exceptions. But on the other hand the chromatic and the characteristic colour thresholds in our observations are somewhat higher.

This regularity of disagreement in colour thresholds may have been due to one or both of two causes. In the first place Aubert and Von Wittich varied the visual angles by employing the method of departure and gradual approach, and this of course implied that the observer knew beforehand what colours to expect at the extreme distance. They record no precautions used to obviate this influence, and the presumption is that they took none. But surely such a mental preparation would have a tendency to bring the characteristic colour thresholds below what they should be if the observer had no means of forming an opinion beforehand of what to expect. It would even have the same influence on the chromatic thresholds, at least when, after a few experiments, the observer would have become familiar with the transitional colours through which each colour passes from the achromatic to the characteristic threshold. Secondly, Aubert employed fewer colours than we did. He used about five or six different tones, among which the observer had to choose, whereas in our experiments under daylight illumination ten different colours were examined. The difficulty in deciding accurately the characteristic threshold would increase with the increase in the number of eligible judgments and would effectually raise our characteristic threshold

On inspection of Von Wittich's figures it is seen that our results on the chromatic threshold correspond fairly well with his results on black ground at continuous view. The discrepancies which occur might easily be accounted for by any of the several influences already cited, or by their combined operation. Von Wittich does not clearly distinguish the chromatic from the characteristic colour threshold, so that we cannot compare our results on the latter with his.

The generally lower achromatic thresholds in our experiments than in either Von Wittich's or Aubert's may be explained largely by the greater accuracy with which we were able to measure the threshold sizes. Apparently Aubert and Von Wittich employed a method of measurement which provided for the recognition of a difference not less than one-fifth of a centimetre. This is seen especially in the results of Von Wittich, where the threshold angles calculated upon the basis of the registered distances make regular advances *per saltum* from 1′ 4″ to 1′ 14″, 1′ 23″, 2′ 17″, etc., in a way which would seem to indicate that the transitions in sizes had not been observed more minutely than as I have suggested. Our own apparatus was designed and constructed so that the transitions could be made in the most gradual way and the slightest changes registered with accuracy to the two-thousandth part of an inch, or even to the four-thousandth part, by estimating halves and quarters of the degrees on the disc. This was quite easy to do from the fact that each degree was about one-eighth of an inch in size.

Von Kries, in a chapter headed "Change of colour in small objects,"* touches upon one aspect of the problem of the space-threshold of colours. He is not dealing at length nor experimentally with it at all, but he gives a concise statement of some of the results which other men such as Von Wittich have reached in his historical account of this and allied problems. He lays particular stress upon the reciprocity which obtains between intensity and spatial size in the threshold perceptibility of colours. In other words, a coloured surface to be just seen as coloured must be larger spatially in proportion as the intensity decreases, and conversely the most minute size of coloured surface may be seen as coloured if the light is sufficiently intense. This mutuality of support between intensity and extension in regard to the first perceptibility of colour, besides being applicable to continuous surfaces, includes, he also observes, the case

* Gesichts Empfindung, p. 87.

of discrete points, as in the phenomenon which Fick described.* Not only will increased intensity secure the perceptibility of the colour of a smaller coloured surface but it will also increase that of a smaller number of discrete colour points, which according to Fick's observation co-operate with one another to produce a colour impression although they may severally be indistinguishable as coloured.

Von Kries has also noted that by decrease of the visual angles coloured surfaces pass through transitions of tone until finally at very small visual angles they lose all colour quality. He has traced the transitions for some colours. Red, he has noted, becomes colourless, but under circumstances that make it very difficult to observe, the limits of visibility usually coinciding with the loss of its colour quality. This is quite in conformity with our own experimental results. Orange, he says, appears red before becoming colourless. With us this took place generally in regard to orange on a black ground, although there were cases where particular observers saw orange as coloured just as long as it was visible at all. Yellow simply passes into white. Green no. 1 becomes white without the intermediate stage of yellow. Green no. 2 at the smallest visual angle appears greener (with some blue) and goes into white by way of yellow-green. Blue becomes colourless without change in tone. Violet becomes reddish at a small angle. These changes of course are for direct vision ; for indirect vision the transitions are in some cases quite different.

Charpentier† has done some work which, though not bearing directly upon this problem, is suggestive. He has in the article referred to examined into the relationship of light intensity of coloured surface to the size of surface necessary for the perception of the surface as coloured. He sought to free his colours from the adulteration of ordinary pigments and to render them as nearly as possible spectrally pure. For example, he obtained his blue by the interposition of cobalt-coloured glass and glass coloured with oxide of copper. The first lets pass only the blue and the red rays, intercepting the green, while the second lets pass only the blue and the green, intercepting the red rays ; the combined effect was to produce a blue resultant of tolerable purity, He had difficulty in producing a spectrally pure yellow (a difficulty which we also encountered in

* Pflüger's Archiv, Bd. vii, p. 152.

† "Sur la quantité de lumière nécessaire pour percevoir la couleur d' objets de différentes surfaces." Comptes Rendus, 1881, 1re Semestre, p. 92.

our work), and finally adopted as standard yellow a combination of all the rays which gave a predominant yellow element. This endeavour of Charpentier's to obtain pure colours was an advance in the treatment of this and kindred problems beyond the unscrutinizing haphazard adoption of pigments which had characterized some of the early work. The principle on which he worked is largely the same as we adopted, although a greater variety of absorbing media was employed in our experiments.

III.

METHOD AND APPARATUS.

The survey of the historical rise and development of the problem of the space threshold of colours is now completed. I will proceed to explain our own method of treating the subject experimentally. The apparatus which we used is somewhat complicated. It was designed by Dr. Kirschmann, to whom also especial thanks are due for supervision and suggestions throughout the course of the investigation.

In the first place it was thought desirable to conduct the experiments in a dark chamber with complete exclusion of daylight and to employ only artificial and therefore controllable illumination. Since our aim was to discover the effect of colour contrast upon the spatial threshold it became necessary to exclude, as far as possible, all such disturbing influences as would undoubtedly arise from the contrast of light intensities under conditions of ordinary daylight. Thus in using daylight we should have no means of controlling the absolute light intensity which illuminates the pigments employed, but by employing artificial light we should be able to equalize the apparent light intensity reflected from the coloured surfaces and so to rule out fairly completely the influence of intensity contrast. It is true indeed that in daylight the absolute light intensity shed at any time would be the same for both of the contrasting colours. But it does not follow that the apparent intensity, *i.e.* the light intensity given off from the pigment surfaces, would be equal. Nothing is more patent than the fact that almost any two pigments subjected to the same illumination, *e.g.* daylight, do not possess the same light-reflecting power, but that one will appear brighter than the other. We can make them of the same brightness by changing the illumination of the one while the other remains constant, and this can be done either by the interposition of some obscuring medium or by moving the source of light. The former method is not satisfactory because it usually introduces further complications in the nature of a change of colour, and hence impairs the value of the results. The other method of control, varying the distance of the source of illumination for one of the surfaces, is impracticable if we use daylight. Two sources of illumination are therefore necessary and one or both of these must be moveable to enable us to rule out all intensity contrast that is irrelevant to the problem and a disturbing influence. These conditions, we thought, could be best fulfilled by carrying on the whole investigation in a room from which daylight was wholly excluded, and where our sources of illumination were incandescent electric lamps of measurable illuminating power and arranged in a certain manner to be presently described. (See Fig. 1.)

The central part of the apparatus upon which the two sets of contrasting colours were set up consisted of a blackened table with two parallel vertical planes, also black, standing upon it at either end and about two feet apart. On the further plane was fastened a revolving disc, covered in sectors by the different pigment papers which were used as the foundation for producing, with various combinations, the several approximately pure spectral colours that we wished to examine. The nearer plane had an opening cut in it which was covered by a brass diaphragm (Fig. 2) with an adjustable square opening. The coloured light from the pigment papers on the further plane was admitted through the opening in varying quantities according to its size. The diaphragm consisted of two brass plates laid together, each having a square opening of two and a half inches diagonal, cut so that the diagonals were horizontal and vertical. The front plate was made to slide in metal grooves attached to the second plate, which was itself permanently fixed to the standing plane. A stationary screw, attached to the permanent plate and passing through a cylindrical nut on the sliding plate, furnished the means of moving the latter and of thereby controlling the size of the aperture from the zero point, where the two square openings just ceased to overlap, through the length of one diagonal, until they became completely coterminous at the maximum opening. The screw was manipulated by a crank handle and was so constructed that every complete turn moved the sliding plate just one-twentieth of an inch, or in other words increased the length of the diagonal of the square opening by one-twentieth of an inch. Each revolution of the screw, or one-twentieth of an inch increase in the diagonal, was indicated upon a graduated scale by a brass finger attached to the sliding plate. Furthermore, to the handle end of the screw was fastened a brass disc of about two inches in diameter, marked off at the circumference into fifty equal parts. As this disc revolved with the screw a peripheral movement through the length of one degree of its circumference would indicate that the sliding plate had moved one-fiftieth of one-twentieth, or the one-thousandth part of an inch. Starting from the zero point of opening we could thus produce in the diaphragm a square aperture, of which the diagonal measured one-thousandth of an inch. At any stage, besides the integral number of revolutions of the screw, which might be found from the index finger and its graduated scale on the groove, we could read the extra fractional part (in fiftieths of a revolution) from another indicator fixed above the revolving brass disc at a point corresponding to the zero point of its scale, that is, at the point where a whole revolution of the disc, and hence of the screw also, was just completed. We had in this simple arrangement a means of varying by gradual transitions the size without the form of the opening through which the coloured light under examination came, from zero up to a square opening of two and a half inches diagonal. Moreover the gradual increase or decrease was measurable with accuracy to the one-thousandth part of an inch, or to the two-thousandth or even the four-thousandth part, if we estimated halves and quarters of the degrees on the brass disc, which had considerable magnitude. In addition to this extreme accuracy of measurement it will be noticed that the apparatus afforded a simple and speedy method of ascertaining the diagonal size of the opening, for a mere glance at the two scales was all that was necessary to read the registration in terms of revolutions of the screw.

On the diaphragm were placed the ground or inducing colours on which we examined the colour threshold under contrast influence. In some experiments this diaphragm surface was transformed into a colourless ground, white, black or grey, by means which will be indicated further on. It may be mentioned that the two brass plates of the diaphragm were not laid exactly together, but that there was left a very thin interspace so as to permit of the insertion of pigment papers. The object of this was that the inner plate of the diaphragm might be given the same colour as the outer plate and thus a continuous coloured surface be presented throughout the movement.

FIG. 1.

Fig. 2.

Here may be mentioned a nicety of construction which was of material importance to the reliability of the results. It became apparent that in putting the coloured paper on the diaphragm we must avoid exposing any ragged or white edges of paper at the margin of the square openings, lest the clearly defined edges of the cut on the outer plate should indicate too conspicuously the exact position of the aperture about to appear, and thus by leading to an anticipation of its appearance vitiate the results. To preclude this chance of error the pigment paper was first pasted on the brass plates, and then by two continuous cuts with a sharp knife, downwards only and slightly towards the brass edge all round each opening, the portion covering the holes came away leaving no uneven edges to the paper and no trace of white. Through the observing tube in front it was now impossible to distinguish lines where the surface of the upper plate ceased and that of the under plate seen through the square hole of the former began, but the whole appeared to be one continuous and uniform coloured surface. So complete was the disappearance of the lines, that very frequently, in fact generally, it was necessary to point out to the observer the neighbourhood on the coloured surface where the point of light would appear, lest he fall into the opposite error of not seeing the emergence of light when it was already visible, from his attention being directed to another portion of the field of vision. It happened, moreover, not seldom, that after being once seen the point of light would disappear again, in consequence of a slight deviation of the eye's fixation, and it would be only necessary to point out the neighbourhood again for the light to be seen. So much help to the observer was absolutely required to prevent his attention from wandering to all parts of the field of vision. But this could not endanger the results in the same way as the careless exposure of lines intersecting at the exact point where the spot of light was to be expected. It involves all the difference between giving the region in which the thing expected must be sought and locating the very point with mathematical exactness.

We had now our two contrasting surfaces, the inducing surface on the diaphragm structure, the induced seen through the opening of the diaphragm and variable in colour by revolution of the colour disc on the further plane. It has been indicated above that each of these surfaces was illuminated artificially and from separate sources. I will now explain the means by which this was done in such a manner as to rule out the disturbance of intensity contrast. The source of light in each case was an incandescent electric lamp of thirty-two candle power placed in a rectangular elongated box which was blackened inside and out, about five feet in length, with a square opening of eight inches to the side. The top was so constructed as to leave a slit along the full length of each box wide enough to contain the neck of a lamp in a wooden sliding frame. In this way the lamp could be moved through the entire length of the box. The open end of each box was directed towards the pigment surface, either the inducing or the induced, so as to permit the light from the lamp to fall obliquely upon it. The opening on top through which the lamp slid was covered by a lid hinged to the large box, for the purpose of preventing unnecessary escape of light. Since either or both the lights could be moved we had a very easy means of ruling out differences of light intensity in the two contrasting surfaces. All that was necessary was to alter the positions of the lights gradually until with a tolerably open diaphragm the light intensity of the two surfaces appeared equal. The point of equality was approached from both sides, first from above the equal point, where one surface was decidedly brightest, and then again from below, where the same surface appeared evidently less bright, so as to arrive as nearly as possible at the exact mean.

At this stage a difficulty presented itself, which it was of great importance to overcome, namely, how to get pure colours to operate upon. I have already pointed out that the work of Aubert and Von Wittich was gravely defective upon this score, and that ordinary pigments are by no means spectrally pure colours, but are the product in all cases of a mixture of spectral colour tones, and may even, as I have indicated, contain no rays of the kind after which they are named. Moreover, our incandescent lamp light is always somewhat yellowish and in being used to illuminate the pigments must cause a decided adulteration of colour tones even if the pigment colours were otherwise spectrally pure. Hence it was incumbent upon us to procure colours for investigation as free as possible from admixture with other colour tones. In order to meet this difficulty we decided to illuminate our pigment surfaces only by light transmitted through coloured media such as coloured glasses and gelatine films. Such media have the convenient property of absorbing some of the rays falling upon them and of allowing others to pass through. Of course different media will absorb and transmit differently and in varying degrees. Some will only weaken instead of totally absorbing the elements they interfere with and will consequently transmit the entire spectrum with weakened elements in some part.* In such a case an effective remedy is to increase the number of the interposed media until the disturbing elements are completely eliminated or at least eliminated to such a degree that the residue transmitted makes no material difference in the result. Moreover when a film or glass has the power of absorbing $\frac{1}{x}$ part of the blue rays, and it is placed before a coloured surface which reflects $\frac{1}{y}$ part of the blue rays which fall upon it, the joint effect will be that only $\frac{1}{x}$ of $\frac{1}{y}$ of the blue rays is reflected. If, for example, we have a blue pigment which reflects 10% of the red, 8% of the orange, 5% of the yellow, 10% of the yellow-green, 25% of the green, 98% of the blue and 40% of the violet rays, and we interpose before it a combination of blue gelatine and glass which transmits 6% of the red rays falling on it, 5% of the orange, 2% of the yellow, 6% of the yellow-green. 10% of the green, 100% of the blue and 20% of the violet, we get as a final colour result $\frac{3}{5}$% of the red rays, $\frac{2}{5}$% of the orange, $\frac{1}{10}$% of the yellow, $\frac{3}{5}$% of the yellow-green, $2\frac{1}{2}$% of the green, 98% of the blue and 8% of the violet, which indicates a very fair removal of the colour elements that obscured the spectral purity of the blue. In this way by various combinations of films and coloured glasses, using different combinations for the different colours desired, we were able closely to approximate to pure colour for the purposes of our experiments—a feature which was absent in the work of Aubert and Von Wittich. For each colour separately we found by actual trial the colour combinations, which under spectroscopic examination seemed to produce a colour most free from foreign elements. And although in no case did we get an absolutely pure colour, yet we succeeded so approximately that the colours which resulted may fairly be said to have been as good specimens of the various spectral colours as could be produced. At least it is safe to say that the errors which must have arisen from the use of pigment colours simply and without any correction were immensely reduced by this device and that our results are to that extent the more reliable. In the accompanying table (pp. 27 and 28) are shown the various combinations by which the colours employed for our

*Kirschmann, Ueber die Herstellung monochromatisches Lichtes.

experiments were produced according to the method indicated and also the synopsis of their spectroscopic analysis.

There remained a further consideration. The observer must be at some distance away from the coloured surfaces to be inspected, and that distance, as I have hinted in the beginning of the paper, must be constant; otherwise errors will creep in from the fact that the visual size of a surface depends not merely upon its absolute magnitude but upon its distance from the centre of the pupil of the eye as well. How then could we secure immunity from the danger of a disturbance caused by the dim light of the room being interposed between the observer's eye and the illuminated pigment surfaces? It is impossible to so darken a room that there will not remain some small reflected light capable of introducing a disturbance into the observations. It would be undesirable moreover to have the room absolutely dark, because in that case a fresh source of error would arise, from the fact that in total darkness the eye is incapable of holding its fixation for any length of time but moves unconsciously within a range of ninety degrees. This would be especially mischievous in experiments on the non-contrast light threshold of colours, where the front colour surface is not illuminated; since the observer might be looking completely in the wrong direction and not discover the emergence of the small point of light until long after the threshold mark. To overcome this difficulty we employed a long observing tube reaching from the observer to close proximity to the brass diaphragm. The tube was constructed of wood in the form of a truncated square pyramid; the larger end, with a diameter of about six inches, was in the vicinity of the brass diaphragm, while the observer's eye was at the smaller end, which was about two inches in diameter. It was blackened outside and the inner surface was lined throughout with black velvet, which, having a minimum power of regular light reflection, consequently reduced the disturbance of glimmer from the sides of the tube to the lowest possible point. Against the larger end of the tube was placed a black cardboard diaphragm, with a square aperture of about two and a quarter inches to the side, but cut diagonally or diamond-shaped like the opening in the brass diaphragm, the purpose of which was to delimit the size of the inducing colour. For obviously the latter could extend, at least so far as the observer was concerned, no further than to the boundaries of the opening in the observing tube. Over the cardboard diaphragm of the tube was arranged a

Table of Colours.

Position of Frauenhoffer's lines on the scale of the spectroscope.

B 421 (687 $\mu\mu$); C 425 (656.5 $\mu\mu$); D 437½ (589.5 $\mu\mu$); E 455½ (527 $\mu\mu$); b_4 459½ (517 $\mu\mu$); F 473 (486 $\mu\mu$); G 512 (431 $\mu\mu$); H (551) not visible. (393.5 $\mu\mu$)

Colour.	Pigment paper.	Illuminated by light from an incandescent lamp transmitted through			Visible part of the spectrum.	Remarks.
		White tissue paper.	Coloured glass.	Coloured gelatine films.		
1. Blue	Blue.			Blue.	450 to 500 (543.5 $\mu\mu$—445.5 $\mu\mu$)	
2. Violet	Blue.			Purple	460 to violet end (515 $\mu\mu$ to violet end)	425 to 439 also seen dimly red. This with a very narrow slit is not noticeable.
3. Purple	Purple			Purple	450 to red end (543.5 $\mu\mu$ to red end)	Red between 420—430. (694.6 $\mu\mu$ to 626.5 $\mu\mu$.)
4. Orange	Orange		2 orange-yellow	Yellow	432 to 448 (616.5 $\mu\mu$ to 550 $\mu\mu$)	
5. Yellow I	Yellow	1 sheet	1 orange-yellow	Yellow	430 to 456 (626.5 $\mu\mu$ to 525.5 $\mu\mu$)	
6. Yellow II	Yellow		1 orange-yellow	1 yellow and 1 green.	438 to 460 (584.5 $\mu\mu$ to 515.3 $\mu\mu$)	
7. Yellow-green	Yellow		1 yellow-green	Green	446 to 462 (557 $\mu\mu$ to 510.5 $\mu\mu$)	The spectrum ends are very abrupt.

TABLE OF COLOURS.—*Continued.*

Colour.	Pigment paper.	Illuminated by light from an incandescent lamp transmitted through			Visible part of the spectrum.	Remarks.
		White tissue paper.	Coloured glass.	Coloured gelatine films.		
8. Green	Green		1 blue-green	Green	445 to 470 (560.5 $\mu\mu$ to 492 $\mu\mu$)	
9. Blue-Green	Blue-green		1 blue-green	Green	448 to 478 (550 $\mu\mu$ to 477.5 $\mu\mu$)	
10. Red	Red		1 red		423 to 435 (671.5 $\mu\mu$ to 600.5 $\mu\mu$)	
11. Grey I (yellowish).	Grey				425 to —— (656.5 $\mu\mu$ to ——)	Full spectrum, greatest intensity at yellow.
12. Grey II (greenish).	Grey			Blue	440 to —— (479.3 $\mu\mu$ to ——)	Maximum intensity at green.

shutter, which could be used to shut off all the light from the illuminated pigments and thus to give the observer's eye a chance to rest between observations. In our experiments it was of course necessary to take the further precaution, after opening the shutter, of pausing briefly to allow the eye to accustom itself again to the bright light and colour. Not to use this precaution would be to introduce the disturbance of successive brightness contrast, just as to neglect to close the shutter frequently would admit the vitiating influence of retinal fatigue.

The small end of the observation tube was kept stationary at the chosen distance from the brass diaphragm. But because of the fact that the opening in the brass diaphragm could be enlarged only on one side, it became necessary to make the larger end of the observing tube also moveable, in order to keep the brass diaphragm opening always in the centre of the field of vision as delimited by the diaphragm of the observation tube. It would not do, however, simply to make the end of the tube move at the same speed as the sliding diaphragm plate, for this would be too fast for the purpose, which was to keep the induced surface marked out by the brass diaphragm opening always in the centre of the field of the inducing or ground surface. For it is evident that the centre of the increasing square aperture in the changing diaphragm moves exactly half as fast as the moving plate which effects its increase. This we can see if we consider that at the zero point the centre coincides with the moving angular point of the plate, but that when the latter has moved any distance the centre is exactly half-way between the zero or starting-point of the moveable plate and its latest position. In order to secure a movement of the observation tube of exactly the desired speed we introduced into the apparatus a very simple leverage device which accomplished the result automatically. We employed an arm twice as long as the distance of the diaphragm plate from the observation tube; one end was fastened to the sliding plate of the brass diaphragm, while the other was pivoted to the table upon which the observation tube stood. From the middle of this arm an attachment by pivot was made with the end of the observation tube. Then as the plate was moved by the operator the arm turned round the pivot on the table, drawing from its middle point the end of the observation tube. This contrivance completely served the purpose of securing an automatic steady shifting of the tube, so that the colour transmitted through the opening in the brass diaphragm was kept precisely in the centre of the inducing ground as delimited by the diaphragm of the observation tube. We were thus enabled to escape from the irregularities of our preliminary method, which was independent manipulation by the operator. The arrangement worked in all respects satisfactorily. So little did the extra weight of the tube obstruct the rotation of the screw which moved the diaphragm plate, that the difference in the ease of the movement was scarcely perceptible to the operator.

There are a few other considerations which I must briefly dwell upon in order to give a full explanation of our experimental equipment. We made some experiments with a black ground, the results of which have been already given in this paper, by way of comparison with those of Von Wittich and Aubert. Although superior to the work of these experimenters both in accuracy of measurement, as can be seen from the description of our apparatus, and by reason of the employment of a black ground that is practically constant, they were not free from errors due to the other causes which I have alluded to in discussing the results of Von Wittich and Aubert. All the disturbances arising from the unequal light intensities of the pigments, when illuminated by daylight whose intensity is uncontrolled and uncontrollable, remained. In order to get a fair approximation to the threshold perceptibility of colours on a black ground it would be necessary not only to use a true black (discarding black paper which reflects considerable light) but also to eliminate any inequality among the different colours of brightness contrast with the black ground. Without this precaution we could not be sure how much more for one than for another colour our judgment was affected by intensity contrast. But if we remove this inequality by making all the colours equally bright, then, although we cannot claim to have eliminated intensity contrast from our conclusions, we can safely claim that we have made it a constant quantity for all the colours. It will enter to exactly the same degree for each colour, since the ground is constant and the brightness of the colours the same. To secure the total removal of intensity contrast it would be necessary, not only for the relation of the brightness of the colours to the ground to be constant, but that that relation should be equality. The ground should have the same brightness as the several colours. This could be effected in the case of a colourless ground only by making it grey and with a system of controllable illumination such as will be described presently. First, however, I will explain the method in which we conducted our experiments with an absolute black ground and sought to reduce the errors of uncorrected pigments and their uncontrolled brightness intensities to a minimum, while converting the unavoidable intensity contrast into a constant and evenly distributed influence for all the colours.

In explaining our mechanism for regulating the contrast in the case of a black ground, as employed in its final form, I shall have occasion at the same time to refer to the method by which we sought and practically secured an immunity from brightness contrast in our experiments with a coloured ground. In fact the latter is the key to the former and the explanation of one will suffice for both.

It has already been said that we discarded daylight illumination for the reasons advanced. The colours which we employed for experiments with a black ground were twelve in number. Their composition I have sufficiently described in the schedule of colours (p. 27) giving their spectroscopic analysis. They were the same colours at the same brightness intensities which we also employed in our experiments with coloured grounds, and were exceptionally successful approximations to pure colours. I will now show how with these colours and under artificial illumination we secured, first the absence of intensity contrast for all the colours on coloured grounds, and secondly from this vantage ground a basis for the limitation of intensity contrast in the case of a black ground to a constant quantity for all the colours.

The diaphragm surface being illuminated by the left-hand lamp at a certain position in its box, the diaphragm was opened to about half its extent, and the other lamp which illuminated the colour on the colour disc (*e.g.* blue) was adjusted to such a position that the observer at the tube judged the light intensity of the blue to be exactly the same as the light intensity of the diaphragm surface. This judgment was, of course, not given instantly nor at random, but was reached gradually, both from below, where the blue seemed the darker, and from above where it seemed the brighter. The position of the shifted lamp when the two coloured surfaces were judged equal in brightness was marked. The similar positions of the lamp for green, red, yellow, etc., throughout the entire set of colours were also marked, and thenceforth always used in the composition of the several colours respectively. Thus, by this simple means, the light intensities of the colours for examination were all equalized with fair exactness. In other words by this practicable method we eliminated for experiments with coloured grounds the influence of brightness contrast.

It is obvious that the success of the above device is dependent upon the reliability of the observer's estimation of equality, but the chances of error were minimized by testing the judgments of one person by those of others. Peculiar difficulty, moreover, in judging equality of light intensities occurs where the surfaces examined are coloured. It then is necessary to abstract attention from the colour altogether, and give it solely to the brightness of the light, a very difficult achievement at first. However, it was proved that the difficulty is overcome by a little practice, and we found in fact that the independent estimates of practised observers were in very close correspondence.

All that was necessary now to render constant the brightness contrast between the various colours and a black ground was simply to convert the coloured ground into absolute black without disturbing the intensities of the induced colours. This was expeditiously and thoroughly done by enclosing and shielding from reflected light by means of black velvet the interspace between the end of the observation tube and the brass diaphragm which was to be converted in black. This served practically the same purpose as the annex to the tube described in connection with our daylight experiments earlier in this paper. It was not found necessary to employ the annex because of the fact that the experiment room was now dark, and it required very little further obscuring in addition to the removal of the lamplight to produce a complete black surface on the diaphragm. In this manner, with such an absolute blackness of surface and with colours all of uniform brightness intensity as well as severally purified from the adulteration of foreign colour tones, it became possible to remove from our observations the multitude of obscure immeasurable influences such as vitiated the results of previous investigations.

In the formation of a grey ground we made a departure from the method of our other experiments. We of course retained the lamp illumination of the colours in order that they should be the same as those experimented with on the other grounds. But we encountered a difficulty in securing a good grey ground by the same method of lamp illumination as we used for the coloured grounds, owing to the fact that the light from the incandescent lamp had a decidedly yellowish tinge and shining upon the best grey pigments produced a yellowish grey ground. It was found next to impossible to obtain absorbing media which would remove this tinge of colour without introducing some other colour element in its place. The nearest approach that we could make to a grey by the use of absorbing media was our grey no. 2, described in the table giving spectroscopic analysis of the colours, and this was somewhat bluish just as the grey no. 1 was somewhat yellowish. In order, therefore, to avoid this difficulty and obtain as pure a grey as possible, we decided to use for the grey ground a daylight illumination of a grey paper surface covering the diaphragm. To secure this without interfering with the lamp illumination of the colours on the further plane, we constructed a shaft, eight inches square and nine feet in length, interior measure, which we used to conduct the daylight from outside to the vertical surface of the brass diaphragm. It was made to bridge the distance from the only window of the room, about seven feet from the floor, to the stand, which was on a level with the lower part of the brass diaphragm, or about two feet and a half from the floor. One end was inserted into a square aperture in the shutter which darkened the window, fitting as closely as possible without interfering with ease of insertion. In order to support the shaft and also to render it more easily manageable from the floor there was a platform attachment to the aperture in the window-shutter, made in box form like the shaft itself, but without a top, and large enough to enclose the end of the shaft. This support was obliquely placed upon the shutter and pointed downwards as the shaft itself did towards the apparatus beneath. The light which would have found its way through the small unavoidable crevices in such a structure was effectually shut out by means of a black cloth screen over the place of junction. The shaft was blackened outside, and inside was painted white throughout. The light was reflected into the shaft by a large mirror outside the window, and the intensity of the light so thrown upon the grey paper was controlled by means of a second reflector, placed below the lower end of the shaft in such a way that the light coming diagonally and from above upon the grey surface could be deflected and thrown perpendicularly upon it. This arrangement was of use on dark days to bring the brightness intensity of the grey up to the same standard of brightness as obtained uniformly among the

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artificially lighted colours to be examined upon this ground. On brighter days the mirror could be changed so as to vary the obliquity in the incidence of the light, and thereby the daylight illumination of the grey could be controlled and approximated to the standard brightness of the colours. On very bright days it was found that the second mirror was not necessary at all, since the light as it came from the shaft was sufficient to produce the required intensity of the grey.

By this expedient of controlled daylight illumination for the ground we were able to secure a fairly good grey, though it sometimes had a bluish tinge. The method was only tentative, but it was found to serve the purpose better than any other discoverable at the time. One grave disadvantage under which it laboured was due to the fact that daylight illumination is somewhat unsteady, being subject to the overshadowing of passing clouds. We sought to eliminate this influence so far as possible by conducting the experiments with the grey ground on cloudless days or when the sky was uniformly cloudy. If again the operator noticed a sudden change in the brightness of the grey he paused in the experiment, closing the shutter of the observation tube until the ground regained its previous intensity.

In concluding the description of our apparatus it must be mentioned that the observer was behind a large black cardboard screen through which the observation tube passed. The screen extended on either side of the tube for at least three feet as well as above and below it, so that the observer was completely precluded from receiving any hint as to the nature of the colour combination introduced from the sight of coloured light reflected from the walls. The only way of seeing the coloured light was through the observation tube itself, after the intervening shutter had been removed.

It may be well before passing on to a discussion of our experimental results to give some explanation of the tables and charts. The former speak for themselves. They are simply the classified record of our averaged results on the three discriminated space thresholds. In the first main section (I) are given the results for the achromatic threshold, in section II those for the chromatic threshold, and in section III those for the characteristic threshold. Under each section we have specified always in thousandths of an inch the diagonal measurements of the diaphragm opening. On this basis we calculate the threshold size in the form of visual angles according to the following simple method :

Fig. 3.

In the accompanying figure (Fig. 3) let A represent the length of the diagonal of the diaphragm aperture at any point which forms the threshold, the distance B being fixed for all sized apertures (172 centim.) as the permanent distance of the observer from the diaphragm. The angle x which the diaphragm diagonal subtends at the observer's eye may be calculated by the following formula :

$$\text{Tan } \tfrac{1}{2} x = \frac{\frac{A}{2}}{B}$$

$$\therefore \text{ Log tan } \tfrac{1}{2} x = \log \frac{A}{2} - \log B.$$

The results of such calculations have been placed in the third sub-column under each main section where there are three sub-columns, or in the second where there are only two. In some cases we have also calculated the visual angles on the basis of the diameter measurement of the diaphragm aperture. These are given in the second sub-column of the three, and were obtained from the diagonal calculations by using the relation of the diameter of a square to the diagonal, which is $1 : \sqrt{2}$. Hence, the visual angle subtended by the diameter being represented by y, while x stands for the diagonal visual angle, we have :

$$\text{Tan } \tfrac{1}{2} y = \frac{\tan \tfrac{1}{2} x}{\sqrt{2}}$$

$$\therefore \text{ Log tan } \tfrac{1}{2} y = \log \tan \tfrac{1}{2} x - \log \sqrt{2}$$

$$= \log \tan \tfrac{1}{2} x - \tfrac{1}{2} \log 2.$$

IV.

RESULTS AND THEIR INTERPRETATION.

Achromatic Thresholds.

(1) Black Ground.—By reference to the accompanying tables, the general relationships which obtain among the achromatic thresholds on a black ground will be manifest. Tables I, II and IV contain, under section I, the results of the achromatic thresholds for three observers respectively, Dr. Kirschmann, Mr. Preston and

Mr. McCallum. The tables representing the observations of Dr. Kirschmann and Mr. Preston were based upon the results obtained from several sets of observations, as many as six in the case of Mr. Preston and three in that of Dr. Kirschmann, whereas Mr. McCallum's table represents only one series of observations. It is obvious, therefore, that where there are any serious discrepancies between Mr. McCallum's observations and the joint testimony of the other two, the presumption is that their sensibility is more accurately represented than is that of Mr. McCallum.

It will be noticed by reference to Tables I, II, III and IV that the colours which, when viewed on a black ground, have lowest achromatic space thresholds are blue and blue-green. The maximum point is attained by the red, while the colours intermediate between red and blue show a somewhat gradual transition from the highest threshold point to the lowest. The most abrupt break is at the yellow-green, where a sudden deviation upward occurs, more marked in the case of Dr. Kirschmann than in that of Mr. Preston. There is not complete agreement between the observations of Dr. Kirschmann and Mr. Preston in regard to violet, which for Dr. Kirschmann (in our marking) has an achromatic threshold considerably higher than purple, but lower than purple for Mr. Preston and also for Mr. McCallum. It will be noticed also that the achromatic threshold of grey no. 1 is lower than that of grey no. 2 for all observers. A peculiarity of red, as distinguished from the other colours when viewed on a black ground, is that it is always and by all observers seen from the very first as coloured. In other words, whereas the other colours can for the most part and with the majority of observers be seen as colourless points of light or simply as something different from the ground, at sufficiently small visual angles, red is conspicuous as having, at least on a black ground, no strictly achromatic threshold. We have, however, used the term achromatic threshold to embrace the visual angle at which a colour is first seen at all, and hence it may be applied to red as well as to the other colours which are first seen as colourless light. It will be found that all the observations recorded agree in this peculiar behaviour of red on the black ground. Some of the other colours are occasionally seen also coloured from the first, but these instances are probably due to accidental circumstances, since even the same observer will at other times see them colourless. In the case of orange, Mr. Preston sees it always coloured, but Dr. Kirschmann and

Mr. McCallum both see it first as colourless light in the majority of their observations, though in some cases, like Mr. Preston, they see it coloured from the first.

It is a noteworthy fact that the region of highest achromatic space thresholds is the red end of the spectrum, and the region of lowest thresholds is that which embraces the other end of the ordinary spectrum, from blue-green to violet, with purple as a mean between the two. This position of purple coincides very appropriately with its known spectral relationship to red and violet, as a transition between these two ends of the ordinary spectrum. This is shown in the "inverted spectrum,"* where purple, which does not appear at all in the ordinary spectrum, is seen as the middle colour, while red and violet are its immediate neighbours on either side.

It might at first seem rather peculiar that the lowest achromatic thresholds should be found in the blue, blue-green and violet region, as these colours are usually regarded by us as less bright than such colours as orange and red, which, according to our experiments, have a much higher achromatic mark. But we must not forget that the colours were reduced so far as possible to the same brightness, so that their comparative positions as regards threshold visibility do not rest upon a basis of brightness, but are due to some other cause inherent in the quality of the coloured light and in the nature of the sensibility to which it appeals. A graphic representation of the relations of the achromatic threshold on black ground is given in Curve I of Figs. 4, 5 and 6, the latter representing the results of an average of observers.

(2) Grey Ground.—If we consider the achromatic threshold results on a grey ground we notice in the first place that the thresholds are, as a whole, considerably higher than those obtained on a black ground. This was to have been expected from the great brightness contrast which was in play in the experiments with a black ground. The sole object, indeed, of performing the experiments on the grey ground was to secure as far as possible an immunity from this contrast in brightness between the colour and the ground. Hence, if our measures were at all effective in achieving the end for which they were devised, we should look for an increase in the threshold sizes.

*Colour Saturation and Its Quantitative Relations, by A. Kirschmann. (American Journal of Psychology, Vol. vii., No. 3, page 389.)

Examining Tables V, VI and VII (Sec. I) we notice tha the relationships of the achromatic thresholds among the colours are considerably altered from those which we found to obtain on the black ground. Instead of red being the region of highest achromatic space thresholds and blue that of the lowest, with the intermediate spectral colours forming a graduated series between, we find now a tendency to decrease among the red thresholds and to increase among the blue. In fact, so great is the change that red has the lowest threshold of all the colours on the grey ground, though not much lower than some colours of the blue-violet region. We notice, however, that in the region from orange-yellow to green, inclusive, the same mutual relations of the parts hold good as on the black ground.

The only serious alterations occur in the thresholds of the colours at the ends of the spectrum. What can be the explanation of this discrepancy between the results on a grey and those on a black ground? I have already pointed out that in both cases the colours were of equal intensity as light, so that their brightness in relation to the ground should in both cases be uniform for all the colours. It cannot, therefore, be attributed to any change in relative brightness. The only difference in the two sets of circumstances was that by substituting a grey for a black ground we removed the inequality of brightness intensities between ground and colour, while retaining the uniform intensity of the latter. But all that we should expect from such a change would be a general elevation of the achromatic thresholds, without disturbance of their mutual relations. We know that the change to the grey ground from the black would modify the brightness intensity of the colours relatively to a percipient, though not absolutely. For if the existence of contrast of brightness means anything it means that the surface observed by the percipient appears to possess a greater intensity than it otherwise would have. Consequently to do away with light contrast would be equivalent to lowering the light intensity of the colours all round—so far at least as the achromatic space thresholds are concerned. But it does not appear that these considerations offer satisfactory explanation of the actual facts observed. It is conceivable that such a change might introduce some modifications into the system of achromatic threshold relations, since equal increases or decreases in the light intensities of colours are accompanied by unequal degrees of change in saturation, and the presumption is that the achromatic space threshold of a colour is quite as much a function of the saturation as of either of the other variables of light phenomena, viz., brightness and colour tone. This supposition, however, even if admitted, does not account for the fact that when a grey instead of a black ground is employed the red region of the spectrum (including orange) is converted from the region of highest to that of lowest achromatic thresholds, while the thresholds of the blue-violet region are raised.

Possibly the explanation of this phenomenon depends largely upon the fact that in spite of our care we were unable to exclude completely the colour element from our grey ground, while keeping it at an intensity equal to that of the colours under examination. The grey which we used was, as has been said, slightly bluish, more conspicuously after exposure to the yellow light of the incandescent lamps, and this bluish tinge in the ground may account for the increase in the blue-violet thresholds and the comparative lowering of those of red and orange. The assumption is here involved that the achromatic space threshold of colours is dependent to some degree upon the contrast relations of colour qualities. Why blue should tend to influence red more than orange is a matter upon which light will be thrown by the exposition of the contrast relations of the actual characteristic and chromatic space thresholds. In the meantime it is sufficient to anticipate the results there reached, and say that we have found blue and red to influence each other's space thresholds more than blue and orange or red and green, the ordinary complementaries. Accordingly, my suggestion is that since the achromatic space threshold may be considered as simply the zero point of perceptible saturation, the same modes of behaviour may obtain here as in the higher degrees of visible saturation. This is not offered as an *a priori* deduction from the relationships of colour contrast, but merely as a plausible theory in explanation of the phenomenon observed; its validity will be strengthened if it is confirmed in the case of achromatic space thresholds on coloured grounds.

Before leaving the observations with a grey ground I wish to mention a fact that is noted in our results. Red, which uniformly appeared coloured when first visible on a black ground, has usually on a grey ground a distinct achromatic threshold. The achromatic threshold is represented for some observers by the curve I in Figs. 7 and 8.

(3) Red Ground.—On reference to Tables VIII, IX, and X (Sec. I), which embody the results of our observations of achromatic

thresholds on a red ground, it will be noticed that the highest thresholds are at the red end of the spectrum, being those of orange, orange-yellow, and purple, while the lowest are in the regions of green and, more especially, blue. The threshold of purple was unusually high, for purple on a red ground is not always seen as a point of light but requires to become of considerable size before it can be distinguished from the red of the ground. Some of our observers, however, did see the purple as a spot of colourless light, and consequently their curves show a lower achromatic threshold of purple than that of Dr. Kirschmann for either eye. The greys occupy a middle place on this ground, and grey no. 1 has a tendency to be somewhat higher than grey no. 2, a significant fact when we remember that grey no. 2 was slightly bluish in tone.

It will be seen that the supposition by which we sought to explain the disturbance of achromatic space threshold relationships among the colours in changing from the black to the grey ground is, according to our anticipation, verified by the results with a red ground. We find that on a red ground the blue and the green have the lowest thresholds, the blue even lower than the green.

(4) Blue Ground.—The results with a blue ground (Sec. I of Tables XVI, XVII, XVIII, XIX, and XX,) are similar to the foregoing. The regions of lowest achromatic space threshold are those of red and orange-yellow. The threshold of orange itself is uniformly somewhat higher than that of either red or orange-yellow. The regions of highest threshold are those of the colours nearest to blue on both sides of the spectrum, viz., green, blue-green, violet, purple, and yellow-green. The greys are closely related, as we might expect, grey no. 1 having a lower threshold than grey no. 2. The principal fact to be noticed in these results is the lowness of the threshold of red which in two out of five of the tables is the lowest point and in the other three is but slightly above that of orange-yellow, the actual lowest for those observers. This would indicate the same tendency already observed in the case of the grey and the red grounds, which is that red and blue are in close correspondence in reference to the facility with which each can be seen achromatically on a ground of the other. It is also rather a peculiar circumstance that orange itself is not influenced to the same degree by blue as either red or yellow no. 1 (orange-yellow) in the direction of low achromatic space threshold. It is natural to suppose that if the achromatic perceptibility of colours is dependent upon the colour of

the ground it will be determined by the ordinary complementary colour relationships. But actual observation does not bear out this supposition, and thus shows us how precarious is all such *a priori* inference from the relations of one limited realm of colour phenomena to those of another yet to be explored. It also shows the necessity and utility of some such provisional hypothesis subject to the evidence of facts. For unless we had anticipated by some vague expectations the way in which the colours would behave under new conditions we should never have appreciated the significance of their non-conformity to colour laws already known.

(5) Green Ground.—On the green ground (Sec. I of Tables XI, XII, XIII, XIV, and XV) the lowest achromatic space thresholds were in the region of the red, except in the case of Dr. Kirschmann, whose observations were not numerous enough to afford a basis for satisfactory comparison with the others. Taking the uniform testimony of the other observers we find that while red has the lowest threshold, the colours of highest achromatic space threshold are uniformly those on either side of green in the spectrum, viz., blue-green, yellow-green, blue and violet. Purple stands in about a medium position. The unanimity with regard to the relative positions of blue and violet is not quite complete. Two observers (Dodds and Creighton) made the threshold for blue lower than that for violet, while all the others gave it as higher. The greys occupy for the most part a middle position, grey no. 1 being below grey No. 2, as was the case when seen on the blue ground. The same state of affairs is thus found to obtain among the achromatic thresholds on the green ground as was noticed on the other coloured grounds. The complementary colour has not the lowest achromatic threshold.

Before concluding the discussion of the achromatic space thresholds I wish to call attention to a significant side light which our experiments in this sphere have thrown upon the ordinary component theories of colour. The Young-Helmholtz theory, for example, is that the three colours, red, green and blue (or violet), are the components both of white light and of all other colours. Each of these original colour sensations is supposed to have a physical basis in the nervous organism, either in the form of special kinds of nerve-fibres which when excited react so as to give rise to their respective colours, or by way of a special kind of visual substance. According to this theory we should expect that one of the composite colours, such as yellow or purple, would at smallest visual angles have a tendency to disappear into one or other of its components, on account of the isolation of the nervous elements stimulated by so small an exposure of coloured surface. But the contrary is the case as we have seen. Instead of vanishing into some other simpler colour element the alleged composite becomes a colourless point of light, a phenomenon which this component theory fails to explain. Again, although it is favourable to such a theory that red does not lose its colour quality on black ground at very small visual angles without becoming forthwith invisible, yet the fact that green and blue (or violet), the other so-called original component colours, do lose their colour quality and become colourless light under the same conditions is entirely inconsistent with it. For if these colour tones are the constituents of white light, we should never expect to find, as is the case, that they lose their colour at small angular sizes and appear as that white light which is supposed to be the product of all three. The same criticism may be urged against Maxwell's theory, which selects for the component colours vermilion, ultramarine and emerald-green.

The achromatic space threshold of the colours on coloured grounds is represented in Figs. 9, 10, and 11 by the dotted curves.

Chromatic Thresholds.

(1) Black Ground.—In our chromatic space thresholds on a black ground as shown in Section II of the corresponding tables, it will be seen that the agreement among the different observers is not by any means complete, although with one exception (Mr. Preston) they are not seriously at variance Apart from Mr. Preston's testimony the results are substantially alike, from purple up to yellow-green inclusive. In the case of green there is considerable disagreement, but after passing green the results again agree fairly well up to blue. Mr. Preston's observations, however, contradict those of the others almost at every point from the red to the yellow-green, inclusive. With so little unanimity among the other observers as to the chromatic space thresholds on black ground we can scarcely find sufficient data for a secure judgment. It is, therefore, difficult to speculate as to the cause of Mr. Preston's noticeable disagreement with his fellow-observers. But we may remark that the strongly emphasized discrepancy in the matter of orange is very probably due to an unusually keen sensibility for orange on the part of Mr. Preston

His six observations on the chromatic threshold of orange were exceptionally close to each other in threshold magnitude and were severally identical with what we named the "achromatic threshold" for orange. In other words Mr. Preston not only saw orange at a very small visual angle but he saw it always coloured as well. Another peculiarity in Mr. Preston's chromatic space thresholds on black ground is that for him, so far as recorded, the colours which have the lowest chromatic space thresholds are the blue-green and violet, no observations having been made by him with blue on this ground.

The other observers are quite unanimous in making red the region of lowest chromatic space thresholds, with a tendency to give a correspondingly high position to blue. They all seem to agree, moreover, in first seeing yellow no. 2 as coloured at a high spatial threshold. Grey no. 2 on this ground, including Mr. Preston's observations, uniformly shows a higher chromatic space threshold than grey no. 1, which is not inconsistent with the threshold position of blue itself since the grey no. 2 was bluish.

One noticeable fact about these chromatic space thresholds on a black ground is that the colours between purple and yellow according to the arrangement of the spectrum are nearly always seen reddish first, while the colours between yellow-green and blue are nearly always seen first as blue.

It should be pointed out that the curves representing the chromatic threshold in our representations are always formed by alternate strokes and dots and are numbered II.

(2) Grey Ground.—Here we notice that the colour which has decidedly the lowest threshold is red in the case of two out of three observers, while for the third it is yellow no. 2. The other comparatively low thresholds are those of orange and perhaps green. The higher markings are for purple, yellow no. 1, and perhaps blue-green, blue and violet. In the cases of blue and violet there is not complete unanimity among the observers, the chromatic space threshold of violet being low for Mr. Creighton as compared with that of blue, and for the writer (Mr. Lane) that of blue being low in comparison with the rest. Mr. Creighton's chromatic thresholds for blue are considerably higher throughout than those obtained by the other two observers. In the violet the reddish elements appeal to him most strongly and at smallest visual angles, as is shown by the fact that the violet always

was seen coloured by him first as red. This, however, is not conclusive, for violet first appears to the other observers also not as bluish but generally as reddish (sometimes with an orange, sometimes with a purple tinge). Nevertheless it is evidence of some individual peculiarities of sensibility. The results of Mr. Creighton's observations, which were averaged to obtain the final representation of the chromatic space thresholds of blue on grey ground for him, were too much in agreement among themselves to suppose that the comparative highness was due to some accidental conditions external to the percipient. I am aware that in our experiments on a grey ground our work was liable to be disturbed by sudden changes in the illumination of the ground which were due to passing clouds. Since, however, the results which were averaged were obtained on separate days, it is unlikely that uniform conditions of disturbance were accidentally present in all instances. There is no doubt, moreover, that the presence of a slightly bluish element in our grey ground would have the effect of raising the chromatic threshold of blue, except where, as in the case of the writer, it was counterbalanced by extra sensibility to blue.

(3) Blue Ground.—In the results on a blue ground we notice the peculiar fact that the regions of lowest space threshold are red and yellow no. 1, the former being in all cases except one (mixed observers) lower than the latter. We notice further that orange has its chromatic space threshold considerably higher than red and for the most part higher than yellow no. 1. After yellow no. 1, following the arrangement in the spectrum, the colours form an ascending series up to blue-green where the thresholds reach their maximum. In the case of green, however, the gradual ascent is broken, green being in one case lower than yellow-green (Dr. Kirschmann's left eye) and in one case higher than blue-green (Mr. Robinson). The former deviation is probably due to the fact that only one set of observations was made by Dr. Kirschmann with his left eye, and that as an offset to his observations with the right eye. The chances are that more observations would have corrected the want of conformity in this instance. The other deviation is not so easily explained. The observations that Mr. Robinson made for this colour were six in number and they all gave a high chromatic space threshold. It can scarcely therefore be accidental, and as the external conditions were the same for all the observers we must ascribe it to a peculiarity in

Mr. Robinson's sensibility. We must not however conclude that he cannot see green at a lower visual angle than that indicated by this threshold, for he sees blue-green *as green* at a very much lower angle. Mr. Dodds also differs from the other observers at yellow no. 1; his threshold for that colour, though not absolutely higher, is yet relatively to orange higher than obtains with them. His observations were quite consistent among themselves and there seems no other reason for the divergence of his results than an individual difference of sensibility. Violet has a higher threshold than purple as a rule, perhaps because of its close qualitative affinity to the blue of the ground. Grey no. 2 is also uniformly much higher than grey no. 1 on this ground, as was to have been expected from its bluish tinge.

The most noteworthy feature, however, about these results on a blue ground is the fact of the lowest threshold being in the red instead of in the orange where we should naturally look for it. The action of colours by contrast in inducing their complementaries upon contiguous or neighbouring surfaces would lead us to expect that in these experiments the emerging surface would tend to appear in the colour complementary to that of the ground. In other words we should expect of a blue ground that it would tend to make every colour at first appear orange or tinged with orange. But as a matter of fact nearly all the colours on a blue ground are seen first as red, even the greens appearing as reddish or brownish (which is a weakly saturated red or orange). It might be said that the facts so far conform broadly to the expectation, because the red or reddish, which is the first chromatic appearance of the colours on blue ground, at such small sizes is practically the same as a faint orange. But we should also expect that orange itself would have a lower chromatic space threshold than red or yellow because of the accentuation of the orange element through contrast. Yet such is not the case. Here as in the achromatic sphere the blue ground seems rather to facilitate the chromatic perceptibility of colours which are not fully complementary to the contrasting ground.

(4) Red Ground.—Turning to our results for chromatic space thresholds on a red ground we notice by reference to the tables that the region of the highest threshold is purple, while the lowest is in blue and the immediately adjacent spectral colours. According to the ordinary complementary relationship of red and blue-green the lowest chromatic space threshold should be that of blue-green. But

although it is low in comparison with purple yet it is unvaryingly higher than blue. With this should be taken into account a fact similar to what was noticed on the blue ground, that the colours are first chromatically seen in nearly all cases as blue instead of the expected green. In Dr. Kirschmann's observations with either eye every colour except purple is seen coloured first as blue, purple being first seen as red. In the set of observations by various observers blue is not so uniformly the vesture of all the colours at their chromatic space threshold, but it prevails. The only colours which are not generally first seen as blue are orange, purple and yellow-green. Orange appears sometimes green, sometimes as a "dirty" yellow. Purple becomes visible as red, yellow-green is seen usually as a dark blue-green, sometimes as green, and three times out of ten even as bluish. It is noteworthy that blue-green and green are seen not as green mainly, when first seen coloured, but as blue. It thus appears that blue-green, the complementary of the red ground, instead of being induced as the first form in which other colours appear coloured on red ground, is itself not first seen as its correct colour. There appears thus to obtain a certain reciprocity between blue and red with regard to the chromatic space threshold of colours on coloured grounds. If the ground is red it seems to facilitate the chromatic space threshold of blue and largely to dispose the other colours to appear first in the form of blue; while if the ground is blue the favoured colour is red. The significance of this peculiar behaviour will appear later. For the present I record it as a remarkable fact which has come out in the course of our experiments.

(5) Green Ground.—The results on this ground compared with those on the blue or the red ground show a general lowering of the chromatic space threshold of purple, but the lowest threshold again tends to be in red with yellow no. 1 not far off. Orange is somewhat higher than either red or yellow no. 1, while it is lower on the other hand than purple. Yellow no. 2 is only moderately high, slightly above orange. Yellow-green is of course very high, as might be expected from its great similarity to the green ground and the consequent difficulty of distinguishing between them for small sizes of the former. Blue-green likewise tends to be high for the same reason. Blue remains high though always somewhat lower than blue-green. Violet is considerably lower than blue though not as low as purple, except that Mr. Shaw and Mr. Creighton both see it at slightly lower visual angles than purple. In Mr. Shaw's case the angles are

so nearly alike that one cannot say that the deviation from the rule obtaining among the others is not merely an accident of circumstances and due to an unusual condition of his sensibility in some of his observations. Mr. Creighton's deviation also can perhaps scarcely be attributed to any other than accidental causes, for while some six observations by him of each of the two colours, violet and purple, give very similar readings for the former they do not manifest the same consistency in the case of purple, the figures being both higher and lower than any of his readings for violet. The greys stand on a green ground very much as they do on a blue ground, relatively to each other. Grey no. 2 is uniformly for all observers higher in chromatic threshold than grey no. 1.

The peculiar feature which has been noted in regard to the results on other grounds is repeated here, that although a slight lowering of the chromatic space thresholds of colours complementary to the ground may be noticed, yet there is a greater lowering still in the threshold of some other colour—red in the case of blue and green grounds and blue in the case of red ground.

I may mention that in our graphic representation for coloured grounds we have in the case of blue ground (Figs. 10 and 11) given only the curve representing chromatic and achromatic thresholds for one observer, in order that the figure may not be too complicated In the figure for green ground only the characteristic threshold is represented, for the same reason.

Characteristic Thresholds.

(1) Black Ground.—Our characteristic space thresholds on a black ground show a very nice agreement between the various observers. As may be seen by a glance at Section III of Tables I, II, III and IV, and Curve III in Figs. 4, 5 and 6, the lowest space thresholds are respectively those of red, yellow no. 2, and the region of blue and blue-green. On the other hand the spectral regions of highest characteristic space thresholds are orange and orange-yellow (yellow no. 1), yellow-green and violet, which last, however, is not for all observers among the highest. Purple and green have medium thresholds. The results are not exactly in agreement as to the relation of orange and yellow no. 1. Mr. Preston sees orange at a very low threshold, in consonance with his results for chromatic space thresholds on a black ground, from which it appeared that he was very sensitive to orange. Mr. McCallum's results and those of

the mixed observers indicate that the orange threshold for them was higher than the threshold of yellow no. 1. However. we cannot permit either of these to decide the actual threshold relationship of orange and yellow no. 1 in the face of the other two results which reverse the order given by these. For Mr. McCallum's results are for only one set of observations, and although strongly corroborating the other results where agreeing with them, yet where disagreeing with them it cannot be taken as evidence in rebuttal, because subsequent observations might easily have transformed the disagreements into uniformity. A single set of observations could scarcely be free from liability to accidental deviations. There is also a large margin of variability, possible and habitual, in the judgments of every observer on these matters, and it is obvious that to adopt any one of these varying judgments as representing the average of them all is to be in danger of greatly distorting the threshold representation. Hence we cannot put serious emphasis upon the contradiction by Mr. McCallum's results of those of Mr. Preston and Dr. Kirschmann, which are based on the average of several sets of observations. The evidence of the mixed observer's results we cannot consider conclusive against what is shown by the others. I may here remark that these results are a combination of four single sets of observations by four different men, and it is necessary to notice that there was not the same degree of agreement between them that was to be found in the several observations of the same observer. Individual differences in sensibility, accentuated by accidental deviations that are due to unaccustomed conditions for observation and uncorrected by multiplied trials, would be very likely to lead to just such divergences as occur in the regions of orange and yellow no. 1. And yet we would maintain the usefulness of such mixed observations. For although it is not advisable to base upon them a refusal to accept the averaged results of several sets of observations by a single observer, yet where they agree, especially where agreement is throughout the largest part of the results, they constitute excellent corroborative testimony.

The results for grey on black ground show that for two of the observers grey no. 2 is seen, in the final form in which it can be seen at all within the limits of the visual angle of the full diaphragm opening ($2° 6' 55.66''$), at a higher mark than grey no. 1. For the other observer they are seen at about an equal marking.

A glance at Curve III of Figs. 4, 5 and 6 will show the coincidence of the different observers in the main. In all three cases we have three marked prominences, a blunt one at the orange and orange-yellow, and a pointed one each in the yellow-green and violet. Between the prominences are depressions in each case, a narrow and pointed one at the yellow (a coincidence of chromatic and characteristic colour threshold), and a wider one from green to blue. Agreement prevails also in the circumstance that the three curves meet each other at the red.

(2) Grey Ground.—On a grey ground the same colours as on a black ground have the lowest thresholds, red, yellow no. 2, blue, and blue-green. There is some difference in regard to the highest markings; yellow no. 1 has alternative readings, one high and another quite low, and the green, which on black ground was a medium colour between high and low in regard to its threshold, has one of the highest thresholds on the grey ground. The explanation of the discrepancy in respect to green and yellow-green is probably to be sought in the fact, that since relatively to the ground there has been a decrease in the intensity of the colours by the loss of brightness contrast, it has become correspondingly difficult to distinguish the green from the yellow-green, and this would result in raising the threshold of green, as is shown in Tables V, VI and VII. A graphic representation is given for two of the observers (Creighton and Dodds) in Curve III of Figs. 7 and 8.

The low alternative threshold of yellow-green may be explained by the passage of a large cloud producing a sudden change in the brightness of the ground, for it will be remembered that we were compelled to have recourse to daylight for the illumination of the grey ground. This darkening of the ground of course effected a relative brightening of the coloured surface, emphasizing its colour quality, especially in the case of our standard yellow-green, which had to be of considerable intensity for the slight yellow element in it to be recognizable.

Another influence besides the one just mentioned assisted in raising the thresholds of the greens. On this ground both green and blue-green generally appeared first as undecidedly bluish or greenish or both combined, and the bluish element clung so long to the green that an observer became disposed to call the colour alternately blue-green and yellow-green. The latter presumably arose from the known indeterminateness of our yellow-green on this ground, combined with the presence in the eye of a negative after-image of the bluish element induced by continuously regarding the coloured surface. I remember quite distinctly in my own observations on green with this grey ground how very difficult it was to see the plain green freed from the strong bluish tinge or its sudden yellowish substitute accelerated by a readjustment of the muscular apparatus of the eye.

There is just such an equivocal relationship between the characteristic thresholds of orange and yellow no. 1; that is, the latter has alternative readings, one high and the other low. The difficulty with all three observers was to distinguish the orange from the yellow. In Mr. Creighton's case the higher marking is probably the more reliable, because his several judgments of the characteristic threshold of yellow no. 1 are prevalently high. Twice out of a total of five observations he does not see it as anything but orange at the full opening of the diaphragm. The lower marking seems to be due to an accidental darkening of the grey ground by a passing cloud, which, of course, effected a relative increase in the brightness of the yellow surface, and thus rendered it more easily distinguishable from orange. For the other two observers, however, the lower markings seem to be the more accurate and representative. Their sensibility is probably somewhat keener than that of Mr. Creighton in the power of distinguishing these two colours from others.

The conclusion, then, that we come to is that the colours having the highest characteristic space threshold on a grey ground are those in the region of orange and yellow no. 1 (with special individual variations), yellow-green and green (with special individual variations), and violet. The lowest points we have already enumerated. The agreement between the results on the grey and those on the black grounds is very generally sustained, but with a considerable enlarging of the thresholds all round on the grey ground as the curves nicely indicate.

(3) Red Ground.—On red ground we find a strikingly unanimous indication that the lowest threshold mark is at blue, and that the colours on both sides of it in the spectrum have increasingly higher thresholds as they are further away from it. (See Tables VIII, IX and X.) This is shown graphically in Fig. 9, Curves III, IV and V, where it is seen not only that the curves are of the same general conformation, but further that they almost come to a point at blue, which is in each case the lowest part of the curve. The

highest marking is reached mainly in orange, yellow-green, violet and purple, while in orange-yellow there is a disposition to decrease, though in no case does it reach the low threshold of blue. The blue-green is for the most part also low. Of the greys, grey no. 2 has the higher characteristic threshold, although they are not really seen characteristically at all within the limits of full opening of the diaphragm, because the colour induction from the red ground is too strong to disappear within so small a visual angle. (I have not represented the greys at all in the curves.)

It will be noticed that very much the same results as obtained on the black and grey grounds in regard to the highest thresholds obtain on the red ground, yellow-green, orange and violet being still the colours with the highest thresholds. The threshold of purple has been raised, however, on this ground, doubtless from the great similarity between purple and red, which would render them not easily distinguishable.

So far as the lowest threshold points are concerned it would seem that the red ground has not altered the condition of things obtaining on the colourless grounds, except by reducing still more the thresholds of colours in the blue-green and blue region.

It must at once strike us as a peculiar fact that on a *red* ground *blue* should be the colour of lowest characteristic space threshold. We should rather have expected that the blue-green would have taken that place from the fact that it is the so-called complementary of red. According to the rules of colour contrast a colour induces upon any contiguous or neighbouring surface its own complementary. Hence, we should expect, on account of the accentuation of the blue-green colour quality by contrast induction from the red ground, that the blue-green would display a marked superiority in the ease with which it makes itself distinguishable at small angular sizes. The influences at play we should expect to be doubly in favour of the early and small-sized threshold of blue-green. For not only is there a positive influence emphasizing the peculiar colour quality of blue-green itself, but there is equally a retarding influence upon every other colour in the form of a blue-green induction, which we must suppose to obscure to some degree their proper colour quality, and to render their definite discernment correspondingly difficult. However, the facts as shown in our experimental results are arrayed against our expectations in this case, and we find as in the case of the chromatic and achromatic space thresholds that the red ground does not apparently facilitate its complementary blue-green in characteristic colour perceptibility according to size, to such a degree as it does another colour which is not its complementary, viz. blue.

(4) Green Ground.—The results on a green ground were somewhat peculiar in several ways. The lowest threshold marking was that of red, as is shown in our Tables XI, XII, XIII, XIV and XV, and in the curves of Fig. 10. Purple is low and in one instance (Mr. Dodds' observations) its threshold magnitude is the same as that of red. Blue also is comparatively low. In fact red, blue and purple, with the two greens (yellow-green and blue-green), which are high in threshold, are the only colours which really had any characteristic colour thresholds at all on the green ground. The curve representation (Fig. 10) indicates not really the characteristic space thresholds for the other colours, but the angular magnitudes at which they were first seen in the final form of their appearance at full opening of the diaphragm aperture. For example, the thresholds for orange in these curves indicate the various points at which orange first appeared as red or purple or orange-red, as the case may have been for the several observers. Yellow no. 1 was seen as orange or orange-red, and yellow no. 2 as orange or orange-yellow. Even yellow-green appeared generally as a yellow-grey and violet either as simply purple or as purple with a slight tinge of violet. Grey no. 2 is uniformly higher than grey no. 1 on this ground in their final form, though strictly they have no characteristic space threshold on this ground.

It will be noticed that although there has been a lowering in the characteristic space threshold of the complementary of green, yet there is another colour, red, whose characteristic space threshold is still lower.

(5) Blue Ground.—By reference to Tables XVI, XVII, XVIII, XIX, XX, and to Fig. 11, we see that on a blue ground the lowest characteristic space threshold is quite decisively that of red. Just as in the case of the red ground at the blue, so here at the red, the curves almost come to a point, which is moreover by far the lowest point. The colours of next lowest characteristic space threshold are respectively purple and yellow-green. The highest points are reached by orange, blue-green, green and the two yellows. In fact orange and the yellows have

scarcely characteristic space thresholds at all within the limits of the full diaphragm aperture, whose visual angle was 2° 6′ 55.66″. Orange is never seen as orange except by one observer, and even with him it was more red than orange. It uniformly appears as red, or red with a very faint tinge of orange. Yellow no. 1 is usually seen as orange-red and yellow no. 2 as orange-yellow. Green is mostly though not always seen as yellow-green and blue-green as plain green. Violet is almost always either purple or red, with a tinge of violet in both cases, only one observer in a few observations making it out as a reddish violet. On this ground the greys were not seen of their proper tone at all. In their final form within the circumscribed limits of largest diaphragm aperture grey no. 1 appeared as orange-yellow and grey no. 2 as "dirty" yellow.

We might be disposed to explain these modifications in our standard colour tones on the blue ground (and on the green ground also as before noted) by saying that the blue ground induces upon the colours an orange-yellow element, which obscures the bluish element in the blue-green (both the saturation contrast of the blue ground and the blue element in the blue-green working in the same direction), makes the green appear yellowish, makes the yellow appear orange, etc. This explanation may hold for the other colours but to orange itself it does not at first sight apply. For we should not expect that the mere accentuation of the orange quality in orange would convert it into another colour, red, instead of effecting an increase in its saturation as orange. However the former is what actually occurs, at least within the limits of the full aperture of the diaphragm. It is futile to seek to explain away this conversion of orange into red at small sizes by supposing that it is due to some minor irregularities in the *fovea centralis*. If such were the case we should expect that so soon as the diaphragm opening was enlarged orange would reappear in place of red. But it does not; the orange remains red and not orange up to the fullest opening of the diaphragm (2° 6′ 55.66″). It appears therefore that this notable phenomenon is not a trifling irregularity of a very limited retinal area but is more probably a property of colour phenomena which must be recognized and reckoned with in any adequate theory of colours. Any explanation like the above, for such a theory as Hering's, which must assume the ordinary complementary relations of the colours to hold at all hazards, would seem to emphasize a factor to which component theories of all kinds have paid but little attention, namely, saturation as an independent variable of light phenomena of equal importance with light intensity and colour tone. It would seem indeed as though there must be here admitted a dependency of colour tone on saturation, somewhat akin to the dependency of colour tone upon light intensity which is called the phenomenon of Purkinje*, or else a violation of the inviolable complementary relationships of the colours. In other words, blue must induce not orange but red, or else the increased saturation of the orange quality leads to a change of its colour tone. It has been found that a considerable change in the brightness intensity of a coloured surface, whether by way of increase or decrease, tends to alter the character of the colour tone; it appears to be equally true that at small visual angles, at least, it is possible to produce a change in colour tone by varying only the saturation intensity while the light intensity remains constant. For those who maintain the component theories of colours this seems to be the only alternative to the admission that two colours may act as complementaries at large angular sizes of exposed surfaces which do not at small visual angles. This latter alternative would of course introduce anarchy and confusion into such a system of balanced complementary constituents as Hering's theory assumes.

But however we may get over the difficulty of explaining the above phenomena on the principles of a component theory, we are still confronted with another phenomenon, which has been cropping out in all our experimental results for the three thresholds. This is not the difficulty of a complementary ground producing a changed tone in one particular colour and an obscuration in others, but that some colour other than the complementary of the ground is seen earlier than the complementary, *i.e.*, at smaller visual angles, at once achromatically, chromatically and characteristically. We notice the same phenomenon with the blue ground as was remarked in the case of the other coloured grounds, namely, that not the complementary, orange, has the lowest characteristic space threshold, but red. If we consider the blue and red grounds together it will be seen that blue and red act much more as complementaries in this matter of space thresholds than do either blue and orange or red and blue-green, the colours which are ordinarily designated complementaries. The blue ground seems to facilitate the minute percepti-

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* Kirschmann's "Colour Saturation and its Quantitative Relations." (American Journal of Psych., vol. viii., p. 394.)

bility of red (both as coloured and as properly coloured) rather than the other colour elements, and the red ground acts similarly upon the blue. We should naturally expect that on a blue ground orange would have the lowest characteristic threshold, but, so far from this being the case, it scarcely has any characteristic threshold at all within the limits of 2° 6′ 55.66″; and on the red ground not the blue-green but the blue is the lowest in characteristic space threshold. What may be the significance of this balanced mutuality of behaviour between blue and red, it is perhaps not in our power to demonstrate. But it would appear that there is a disturbance of the ordinary complementary relationships of the various members of the colour system when the colour surfaces exposed are reduced to small visual angles.

Whatever the real explanation of the fact, it is interesting to note that there is on record a case of monocular colour-blindness in which blue and red were the only elements in the colour system, and that they acted towards each other like ordinary complementaries, just as in our space threshold experiments. The case was investigated by Dr. Kirschmann in the Leipzig Laboratory, and is recorded in his article "Beiträge zur Kenntnisse des Farbenblindheit."* It is the case of Professor A. (numbered case five of the beforementioned article), a man whose left eye was perfectly normal in its colour sensibility, but whose right eye was a dichromate colour-blind, having only blue and red as the foundation of its colour system. The irregularity in the right eye was congenital, and not produced by any accident or disease, as is evident from the fact that some other members of his family were organized very much in the same way. Experiments were carefully made with the spectroscope for the two eyes independently, and it was found that while the left eye was quite normal in its appreciation of the various colours, the right eye was capable of distinguishing only red and blue; all the other colours were seen as various saturations of one or other of these. Orange, for example, as in our space threshold experiments on a blue ground, was seen as red. Careful after-image tests were made with spectral colours, and not only was the colour system for the abnormal eye one-dimensional, based on blue and red, but the blue and red acted throughout as complementaries. Each spectral colour seen as red left a blue negative after-image, and *vice versa*.

*Philos. Studien, vol. viii, p. 199.

Such a condition of things was, of course, rather remarkable, both because of the fact that the one eye was perfectly normal in its appreciation of colours and because the impaired sensibility of the colour-blind eye showed neither more nor less than normal appreciation for red and blue, while at the same time these two colours acted as complementaries. Such an unorthodox behaviour on the part of this colour-blind eye was quite in contravention of the component theories of colour, and among the adherents of the component theories there was a disposition to deny the accuracy of the experiments,* to minimize the importance of the facts disclosed, or even to ignore them altogether. For it is plain that if white light is composed of three constituent colour elements, red, blue and green, as the Young-Helmholtz theory assumes, it is inexplicable how this colour-blind eye could see colourless light at all, having sensibility for only two of the three, red and blue. Again, if red is a sensation which arises from the destruction of a certain kind of visual substance, and blue a sensation arising from the construction of a totally different kind of visual substance, as Hering's theory propounds, it is inexplicable how this particular colour-blind eye could continue long to have sensibility for more than blue alone. For the blue and red of this abnormal eye are exactly the same as those colours in the normal eye, as was shown by careful test. Hence the destruction of the red substance could never be made up by the construction of the blue, since according to the theory they are totally different, with the inevitable result that the red substance must ultimately become exhausted, and, *eo ipso*, the appreciation of red be destroyed. There is no obvious way of adroitly escaping from the difficulty by supposing this a special case where the red and blue are the dissimilative and assimilative aspects of the same visual substance, because this is to assume that red and blue for this eye are different from red and blue for the normal eye, which was experimentally shown not to be the case.

My object in referring to Dr. Kirschmann's paper is to call attention to the remarkable coincidence between the colour sensi-

*Professor Ebinghaus shows this attitude toward the matter in his article on p. 215, Band v, of the *Zeitschrift für Psychologie*. The importance of the above case of colour-blindness can scarcely be set aside in the summary way that Professor Ebinghaus is disposed to use. The observations and experiments were not only carefully and accurately conducted, but Professor A. was an expert opticist, and thoroughly competent to judge the facts presented to him.

bility for blue and red in this colour-blind dichromate and our space threshold results. I have frequently in this paper laid stress on the fact disclosed in our results that coloured grounds do not seem to lower the space threshold of their ordinary complementaries so much as they do that of some other colours, as red and blue. We have noticed that on red ground almost all the colours appear first as blue, and on blue nearly all appear first as red, including even the complementaries themselves of the coloured grounds. We have also noticed that in regard to characteristic space thresholds there appears to be the same disturbance of the ordinary complementary relationships of the colour system as in the other thresholds, and that the evidence for it is even more emphatically unanimous. In our experiments for determining space thresholds red and blue seem to act exactly as complementaries might be expected to act.

It thus appears, so far as our experiments go, that for small angular sizes of coloured surfaces there is a disturbance of ordinary complementary relations, and that for red and blue grounds at small visual angles a condition of things obtains which is somewhat similar to that present in the colour-blind eye of Professor A. The coincidence is not quite complete because there is not absolute failure to appreciate other colour qualities besides red and blue. I would enunciate our conclusions on this question in the following way :—On red and blue grounds, below the limits of the characteristic space thresholds of blue and red respectively, there is a lack of ability in the normal eye to make definite discriminations of the other spectral colour tones and a tendency to confuse them with either red or blue. Thus in a limited sphere embracing only small angular sizes are practically reproduced the conditions of colour sensibility exhibited by the colour-blind eye of Professor A., which form a colour system of one dimension founded on the two colours red and blue.

Further, it is interesting to note that in this peculiar complementary relation between blue and red we have come across what in our estimation is a formidable difficulty to Hering's colour theory, by which all colour phenomena are explained by a threefold antagonism of fundamental colour processes. Black, red and yellow are by him set over against white, green and blue respectively, the former three being the outcome of the destruction of three distinct kinds of visual substance, while the corresponding latter three are the outcome of the construction or assimilation of the same kinds of nervous substance respectively. A complementary relation of colours is inherent in the nature of the optical organism and is the issue of the natural balanced activity of fatigue and repair, destruction of tissue and consequent recuperation. The different sets of complementaries belong to the activity of the different kinds of visual substance. From this theory it would follow that the red process set up in the optical apparatus should originate the green process in a contiguous surface relieved from the stimulation of the red. Applied in particular to our course of experiments we should expect that the red ground, when watched for the emergence at its centre of a small surface differently qualified, would tend to induce upon the emerging surface a green colour. The theory apparently demands so much by its physical resolution of the phenomena of complementary relationships, rendering them rigid and inviolable. But the facts of the case as shown in our experiments are that the red does not induce green but blue. And it appears to us that this actual disturbance of the complementary relationships is a difficulty which Hering's theory is utterly unable to account for.

It might be offered as a plausible explanation from Hering's point of view, and according to his terminology, that the matter is an aspect of irradiation or light induction or negative contrast, by which is meant that a coloured surface tends not merely to induce upon a contiguous surface its complementary colour but also to spread its own peculiar colour quality over it. But our rejection of such an explanation is unqualified. In the first place, if the red tends not only to induce green upon the surface revealed by the opening aperture of the diaphragm but also to irradiate a positive influence of red itself, we are no nearer an explanation than before, having on our hands a weakened saturation of induced green instead of the blue which we wished to explain. For it follows that if the negative induction assumed is slight and not quantitatively equal to the green induced, then as far as it goes it will neutralize the colour quality of some of the green induced, and produce colourless light. This, which is a strict deduction from the theory, would still leave a weak saturation of green and not blue, because of the admixture of the colourless light and the residue of green. Again, if the negative induction is equal in quantity to the green induction, we should have no colour induction at all but simply an induction of colourless light, which is as little like blue as ever. Finally, if we admit the inadmissible, that some different colour result than green could be produced in such a case according to the strict letter of Hering's theory, then we must demand what reason can be shown why the negative induction should lead to a compromise result on the blue side of green instead of on the yellow. There is just as much reason *a priori* for one as for the other, and the fact that the blue is actually the one induced is indicative that there must be some reason beyond the scope of the explanations that the Hering theory can offer. There is a certain so-called parsimony in nature, but scientists must not outstrip nature in parsimony to such a degree as to make everything in nature fit to a few cramped or Procrustean explanations, which have the advantage of brevity and clearness but at the expense of exactness.

Table I.—On Black Ground.

Observer: Dr. Kirschmann.

Colour.	I. Achromatic threshold.					II. Chromatic threshold.					III. Characteristic colour threshold.					Remarks.
	Diagonal of diaphragm opening in 1000ths of aninch.	Visual angle of diameter of diaphragm opening.		Visual angle of diagonal of diaphragm opening.		Diagonal of diaphragm opening in 1000ths of an inch.	Visual angle of diameter of diaphragm opening.		Visual angle of diagonal of diaphragm opening.		Diagonal of diaphragm opening in 1000ths of an inch.	Visual angle of diameter of diaphragm opening.		Visual angle of diagonal of diaphragm opening.		
		′	″	′	″		′	″	′	″		′	″	′	″	
Purple..........	36 1/6	1	17.9	1	50.16	78 2/3	2	49.44	3	59.61	154 1/3	5	32.41	7	50.1	
Red............	57	2	3.77	2	53.62	57	2	3.77	2	53.62	82	2	56.62	4	9.77	
Orange.........	45 1/6	1	39.28	2	17.58	71 1/6	2	33.28	3	36.77	409 1/3	14	41.64	20	46.82	
Yellow I........	38 1/3	1	22.56	1	56.76	77 2/3	2	47.28	3	56.52	456 5/6	16	23.95	23	11.51	
Yellow II.......	30	1	4.62	1	31.38	104	3	44.	5	16.82	104	3	44.	5	16.82	
Yellow-green....	42 1/2	1	31.54	2	9.46	86 5/6	3	7.02	4	24.49	288	10	20.3	14	39.	
Green..........	32 2/3	1	10.36	1	39.52	100 1/3	3	36.1	5	5.61	167 2/3	6	1.13	8	30.71	
Blue-green......	24 1/3		52.41	1	14.1	70 1/3	2	31.49	3	34.23	113 2/3	4	4.82	5	46.23	
Blue...........	22 3/4		49.01	1	9.3	82	2	56.62	4	9.77	133	4	46.46	6	45.12	
Violet.........	25	1	16.1	2	3.3	84	3	0.92	4	15.86	313 1/2	11	15.23	15	54.92	Poorer violet outside than out at full.
Grey I..........	24		51.69	1	13.11	62 1/3	2	14.22	3	9.87	70	2	30.77	3	33.22	
Grey II.........	37 1/3	1	20.41	1	53.72	114 5/6	4	7.28	5	49.78	489 1/3	17	33.95	24	50.5	

Table II.—ON BLACK GROUND.

Observer: Mr. Preston.

Colour.	I. Achromatic threshold.					II. Chromatic threshold.					III. Characteristic colour threshold.					Remarks.
	Diagonal of diaphragm opening in 1000ths of an inch.	Visual angle of diameter of diaphragm opening.		Visual angle of diagonal of diaphragm opening.		Diagonal of diaphragm opening in 1000ths of an inch.	Visual angle of diameter of diaphragm opening.		Visual angle of diagonal of diaphragm opening.		Diagonal of diaphragm opening in 1000ths of an inch.	Visual angle of diameter of diaphragm opening.		Visual angle of diagonal of diaphragm opening.		
		′	″	′	″		′	″	′	″		′	″	′	″	
Purple	$27\frac{2}{5}$		59.	1	23.46	$61\frac{1}{3}$	2	12.1	3	6.8	$129\frac{4}{5}$	4	39.57	6	35.4	
Red	$55\frac{1}{5}$	1	58.9	2	48.14	$55\frac{1}{5}$	1	58.9	2	48.14	$55\frac{1}{5}$	1	58.9	2	48.14	
Orange	$44\frac{2}{5}$	1	35.64	2	15.2	$44\frac{2}{5}$	1	35.64	2	15.2	$132\frac{4}{5}$	4	46.	6	44.5	
Yellow I.	$31\frac{1}{5}$	1	7.2	1	35.04	$54\frac{3}{5}$	1	49.74	2	46.32	$719\frac{1}{5}$	25	49.04	37	10.66	
Yellow II.	$30\frac{1}{5}$	1	5.06	1	31.8	47	1	41.24	2	23.16	$196\frac{9}{10}$	7	2.2	9	59.74	
Yellow-green	$33\frac{2}{5}$	1	11.94	1	41.74	50	1	47.7	2	32.26	699	25	5.54	35	30.94	
Green	$29\frac{2}{5}$	1	3.32	1	27.56	$50\frac{1}{5}$	1	48.12	2	32.9	$304\frac{4}{5}$	10	56.48	15	28.4	
Blue-green	$24\frac{2}{5}$		52.56	1	14.32	$37\frac{2}{5}$	1	20.52	1	53.9	$223\frac{3}{5}$	8	1.6	11	21.1	
Blue	.....	...	...	...	...	.....	...	...	...	...	.....	...	...	...	...	
Violet	25		53.84	1	16.14	$37\frac{4}{5}$	1	21.42	1	55.14	$187\frac{1}{5}$	6	43.2	9	30.22	
Grey I.	$24\frac{2}{5}$		52.58	1	14.32	$51\frac{2}{5}$	1	50.97	2	36.57	$236\frac{1}{3}$	8	31.6	11	59.8	
Grey II.	$28\frac{3}{5}$	1	1.6	1	27.12	$59\frac{1}{4}$	2	7.62	3	0.48	$225\frac{3}{4}$	8	6.23	11	27.64	

Table III.—On Black Ground.

Mixed Observers (4 averaged).

Colour.	I. Achromatic threshold.					II. Chromatic threshold.					III. Characteristic colour threshold.					Remarks.
	Diagonal of diaphragm opening in 1000ths of an inch.	Visual angle of diameter of diaphragm opening.		Visual angle of diagonal of diaphragm opening.		Diagonal of diaphragm opening in 1000ths of an inch.	Visual angle of diameter of diaphragm opening.		Visual angle of diagonal of diaphragm opeding.		Diagonal of diaphragm opening in 1000ths of an inch.	Visual angle of diameter of diaphragm opening.		Visual angle of diagonal of diaphragm opening.		
		′	″	′	″		′	″	′	″		′	″	′	″	
Purple..........	$38\frac{5}{6}$	....	....	1	58.2	$56\frac{1}{2}$	2	2.	2	52.	234	8	24.	11	52.77	
Red............	$34\frac{1}{4}$	1	13.77	1	44.33	$34\frac{1}{4}$	1	13.77	1	44.33	$34\frac{1}{4}$	1	13 77	1	44.33	
Orange........	$42\frac{7}{8}$	....	....	2	10.6	$65\frac{1}{4}$	2	20.54	3	18.75	355	12	46 8	18	1.4	
Yellow I........	$40\frac{3}{8}$	1	26.96	2	2.99	$83\frac{7}{8}$	3	0.76	4	15.48	$279\frac{2}{3}$	10	2.	14	11.	
Yellow II.......	40	1	26.16	2	1.84	156	5	36.	7	55.18	156	5	36.	7	55.18	
Yellow-green....	$32\frac{1}{4}$	1	9.47	1	38.24	$114\frac{1}{8}$	4	5.81	5	47.63	465	16	41.44	23	36.39	
Green..........	$34\frac{1}{8}$	1	13 5	1	43.94	$71\frac{5}{8}$	2	34 27	3	38.17	$136\frac{1}{3}$	4	54.	6	55.78	
Blue-green......	31	1	6.79	1	34.43	68	2	26.46	3	37.13	$121\frac{1}{4}$	4	21.16	6	8.73	
Blue...........	$31\frac{2}{3}$	1	8.21	1	36.46	$115\frac{1}{3}$	4	8.41	5	51.3	161	5	46.6	8	10.	
Violet..........	$32\frac{1}{4}$	1	9.47	1	38.24	$59\frac{1}{4}$	2	7.62	3	0.48	$316\frac{1}{2}$	11	20.69	16	4.06	
Grey I..........	32	1	8.93	1	37.48	93	3	20.31	4	43.28	323	11	35.69	16	23.86	
Grey II.........	32	1	8.93	1	37.48	139	4	59.39	7	3.40	$370\frac{1}{3}$	13	17.64	18	48.04	

Table IV.—On Black Ground.

Observer: Mr. McCallum.

Colour.	I. Achromatic threshold.			II. Chromatic threshold.			III. Characteristic colour threshold.			Remarks.
	Diagonal of diaphragm opening in 1000ths of an inch.	Visual angle of diameter of diaphragm opening.	Visual angle of diagonal of diaphragm opening.	Diagonal of diaphragm opening in 1000ths of an inch.	Visual angle of diameter of diaphragm opening.	Visual angle of diagonal of diaphragm opening.	Diagonal of diaphragm opening in 1000ths of an inch.	Visual angle of diameter of diaphragm opening.	Visual angle of diagonal of diaphragm opening.	
		′ ″	′ ″		′ ″	′ ″		′ ″	′ ″	
Purple..........	40	1 27.6	2 1.8	64	2 14.6	3 14.8	284	10 11.6	14 25.	
Red............	36	1 17.6	1 49.6	36	1 17.6	1 49.6	36	1 17.6	1 49 6	
Orange.........	42	1 30.4	2 7.9	90	3 13.8	4 35.2	588	21 6.4	29 51.	
Yellow I........	44	1 34.6	2 14.	139	4 59.4	7 13.4	477	17 7.2	24 13.	
Yellow II.......	40	1 26.16	2 1.84	156	5 36.	7 55.18	156	5 36.	7 55.18	
Yellow-green....	31	1 6.8	1 34.4	136½	4 54.	6 55.8	458	16 26.	23 15.	
Green..........	28	1 0.2	1 25.2	94	3 22.4	4 46.2	181	6 29 8	9 11.	
Blue-green......	20	43.	1 0.8	96	3 27.8	4 52.4	177½	6 22.	9 0.	
Blue............	28	1 0.2	1 25.2	161	5 46.6	8 10.4	161	5 46.6	8 10.4	
Violet..........	25	53.8	1 16.	72	2 35.	3 39.2	550	19 44.6	27 55.2	

Representation of the Space-Thresholds for Black Ground.

Scale: 1 inch = 10 minutes.
Curve I—Achromatic Threshold,
" II—Chromatic Threshold,
" III—Characteristic Colour Threshold.

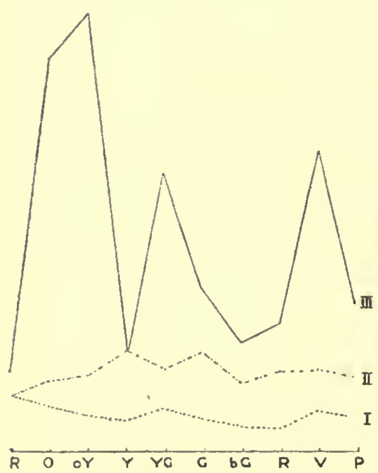

Fig. 4 (Table I).

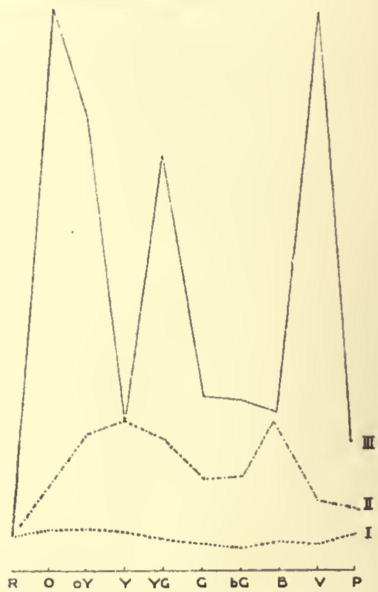

Fig. 5 (Table II).

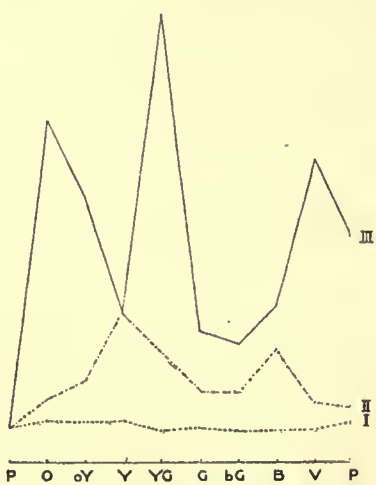

Fig. 6 (Table III).

Table V.—On Grey Ground.

Observer: Mr. Creighton.

Colour.	I. Achromatic threshold.		II. Chromatic threshold.		III. Characteristic colour threshold.		Remarks.
	Diagonal of diaphragm opening in 1000ths of an inch.	Visual angle of diagonal of diaphragm opening.	Diagonal of diaphragm opening in 1000ths of an inch.	Visual angle of diagonal of diaphragm opening.	Diagonal of diaphragm opening in 1000ths of an inch.	Visual angle of diagonal of diaphragm opening.	
		′ ″		′ ″		′ ″	
Purple	71 3/8	3 37.3	103 7/8	5 16.3	205	10 24.45	
Red	41 3/4	2 7.15	74 1/4	3 46.15	129	6 33.	
Orange	55 1/2	2 49.	71 9/10	3 39.	1036 1/2	52 37.12	
Yellow I.	73 1/5	3 42.95	82 3/5	4 14.7	1347 (105)	1° 8 22.94 (5′ 19.8″)	
Yellow II.	48 3/8	2 27.2	59 1/8	3 0.	81 1/4	4 7.46	
Yellow-green	57 2/5	2 54.8	78 1/5	3 58.1	678 1/3	34 27.92	Seen twice at full as plain green only.
Green	51 1/2	2 36.86	73	3 42.4	1341	1° 8 4.66	
Blue-green	53 1/8	2 41.8	85 7/8	4 21.55	128 1/2	6 31.5	
Blue	48 2/3	2 28.	110 3/4	5 37.3	250 3/4	12 43.73	
Violet	44 1/2	2 15.5	75	3 48.4	615 2/3	31 15.3	

Table VI.—On Grey Ground.

Observer: Mr. Dodds.

Colour.	I. Achromatic threshold. Diagonal of diaphragm opening in 1000ths of an inch.	I. Achromatic threshold. Visual angle of diagonal of diaphragm opening.	II. Chromatic threshold. Diagonal of diaphragm opening in 1000ths of an inch.	II. Chromatic threshold. Visual angle of diagonal of diaphragm opening.	III. Characteristic colour threshold. Diagonal of diaphragm opening in 1000ths of an inch.	III. Characteristic colour threshold. Visual angle of diagonal of diaphragm opening.	Remarks.
		′ ″		′ ″		′ ″	
Purple	79¾	4 2.86	99⅙	5 2.	225	11 24.4	Sometimes seen purple-orange.
Red	58	2 56.66	58	2 56.66	130¼	6 33.7	
Orange	70	3 33.2	74¼	3 46.16	109¼ (631⅓)	5 32.8 (33′ 33.6″)	
Yellow I	80¾	4 5.93	87⅕	4 26.5	225	11 24.4	
Yellow II	71	3 36.3	77¾	3 56.75	104½	5 17.25	
Yellow-green	79	4 0.53	83	4 12.86	406⅚ (512½)	20 39.2 (26′ 7.4″)	
Green	73¼	3 45.1	73¼	3 45.1	407½	20 41.24	
Blue-green	70	3 33.2	74½	3 46.9	160¾	8 9.65	
Blue	62	3 8.9	87¼	4 25.7	135½	6 52.75	
Violet	74	3 45.3	89½	4 32.6	465	23 36.39	

Table VII.—On Grey Ground.

Observer: Mr. W. B. Lane.

Colour.	I. Achromatic threshold.			II. Chromatic threshold.			III. Characteristic colour threshold.			Remarks.
	Diagonal of diaphragm opening in 1000ths of an inch.	Visual angle of diagonal of diaphragm opening.		Diagonal of diaphragm opening in 1000ths of an inch.	Visual angle of diagonal of diaphragm opening.		Diagonal of diaphragm opening in 1000ths of an inch.	Visual angle of diagonal of diaphragm opening.		
		′	″		′	″		′	″	
Purple	$65\frac{3}{4}$	3	20.26	$75\frac{1}{2}$	3	49.9	$242\frac{1}{4}$	12	17.9	
Red	$43\frac{7}{12}$	2	12.75	$52\frac{1}{4}$	2	39.1	$86\frac{3}{10}$	4	22.8	
Orange	$55\frac{5}{7}$	2	49.7	58	2	56.66	$535\frac{3}{4}$	27	12.	
Yellow I	$58\frac{5}{7}$	2	58.8	$65\frac{6}{7}$	3	20.53	$(1233\frac{1}{5})$ $257\frac{5}{6}$	(1° 2′ 36.2″) 13	2.26	
Yellow II	$56\frac{2}{5}$	2	51.46	$67\frac{3}{8}$	3	25.25	$181\frac{3}{5}$	9	13.1	
Yellow-green	$69\frac{5}{14}$	3	31.	$77\frac{9}{14}$	3	56.46	$(1459\frac{2}{3})$ $307\frac{1}{2}$	(1° 14′ 6.06″) 15	15.22	
Green	$69\frac{5}{7}$	3	32.3	74	3	45.35	$603\frac{1}{3}$	30	37.74	
Blue-green	$66\frac{1}{2}$	3	22.5	86	4	21.9	$132\frac{1}{8}$	6	42.45	
Blue	$51\frac{1}{6}$	2	36.86	$61\frac{1}{2}$	3	7.33	$146\frac{2}{3}$	7	26.73	
Violet	$64\frac{3}{5}$	3	16.8	$67\frac{3}{4}$	3	26.33	426	21	37.66	

REPRESENTATION OF THE SPACE-THRESHOLDS FOR GREY GROUND.

Scale : 1 inch = 20 minutes.
Curve I—Achromatic Threshold.
" II—Chromatic Threshold.
" III—Characteristic Colour Threshold.

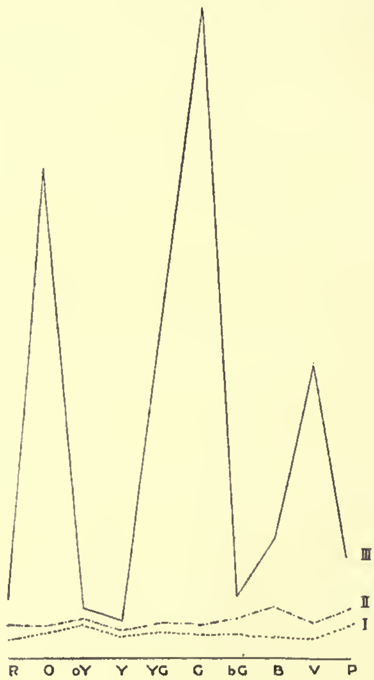

FIG. 7 (Table V).

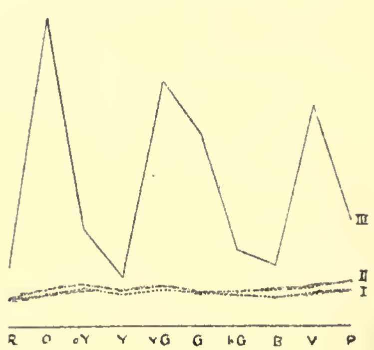

FIG. 8 (Table VI).

Table VIII.—On Red Ground.

Observer: Dr. Kirschmann (left eye).

Colour.	I. Achromatic threshold.		II. Chromatic threshold.		III. Characteristic colour threshold.		Remarks.
	Diagonal of diaphragm opening in 1000ths of an inch.	Visual angle of diagonal of diaphragm opening.	Diagonal of diaphragm opening in 1000ths of an inch.	Visual angle of diagonal of diaphragm opening.	Diagonal of diaphragm opening in 1000ths of an inch.	Visual angle of diagonal of diaphragm opening.	
Purple............	307	15′ 35.14″	307	15′ 35.14″	487	24′ 43.4″	
Orange	86	4′ 21.93″	137	6′ 57.3″	963	48′ 53.32″	Seen first coloured bluish.
Yellow I..........	68½	3′ 28.6″	134	6′ 48.13″	577	29′ 17.58″	" " "
Yellow-green......	77	3′ 54.5″	102½	5′ 12.2″	600	30′ 27.6″	" " bluish-grey.
Green	61½	3′ 7.36″	103	5′ 13.7″	415	24′ 4.12″	" " bluish.
Blue-green........	66½	3′ 22.5″	108	5′ 29.6″	247	12′ 32.39″	" " blue.
Blue	55½	2′ 49.05″	171	8′ 40.85″	171	8′ 40.85″	" " "
Violet............	77	3′ 54.5″	136½	6′ 55.8″	335	17′ 0.44″	" " "
Grey I............	64	3′ 14.95″	295	14′ 58.6″	1230	49′ 36.″	
Grey II	64	3′ 14.95″	124	6′ 17.75″	1336½	1° 7′ 33.6″	

Table IX.—On Red Ground.

Observer: Dr. Kirschmann (right eye).

Colour.	I. Achromatic threshold. Diagonal of diaphragm opening in 1000ths of an inch.	I. Visual angle of diagonal. ′	″	II. Chromatic threshold. Diagonal of diaphragm opening in 1000ths of an inch.	II. Visual angle of diagonal. ′	″	III. Characteristic colour threshold. Diagonal of diaphragm opening in 1000ths of an inch.	III. Visual angle of diagonal. °	′	″	Remarks.
Purple............	$247\frac{3}{5}$	12	32 44	$421\frac{4}{5}$	21	24.82	$799\frac{3}{5}$		40	35.56	
Orange	$122\frac{1}{5}$	6	12.36	$154\frac{1}{3}$	7	50.	$1880\frac{4}{5}$	1	35	21.56	Seen first coloured bluish.
Yellow I..........	$88\frac{2}{5}$	4	29.36	$157\frac{2}{5}$	7	59.44	$1082\frac{3}{5}$		54	57.52	" " "
Yellow-green......	$83\frac{1}{5}$	4	13.42	$154\frac{3}{5}$	7	50 9	(713)		(35	41.32)	" " " and at full opening plain green.
Green	$77\frac{1}{10}$	3	54.84	161	8	10.4	$966\frac{3}{4}$		49	4.68	Seen first coloured bluish.
Blue-green........	80	4	3.68	$160\frac{1}{5}$	8	8.	382		19	31.64	" " "
Blue	$75\frac{3}{4}$	3	50.66	$111\frac{3}{8}$	5	39.	$179\frac{7}{10}$		9	7.36	" " "
Violet	87	4	25.	$196\frac{1}{6}$	9	58.54	$484\frac{3}{10}$		24	35.18	" " "
Grey I............	73	3	42.36	....	.....	.....	$1725\frac{1}{3}$	1	27	35.096	Seen at full as yellowish-grey.
Grey II	$73\frac{4}{5}$	3	44.75	$121\frac{2}{3}$	4	7.74	....	.....	.....	.....	Seen first coloured blue.

Table X.—On Red Ground.

Mixed observers (10).

Colour.	I. Achromatic threshold. Diagonal of diaphragm opening in 1000ths of an inch.	I. Visual angle of diagonal of diaphragm opening. ′	″	II. Chromatic threshold. Diagonal of diaphragm opening in 1000ths of an inch.	II. Visual angle of diagonal of diaphragm opening. ′	″	III. Characteristic colour threshold. Diagonal of diaphragm opening in 1000ths of an inch.	III. Visual angle of diagonal of diaphragm opening. °	′	″	Remarks.
Purple	$98\frac{3}{20}$	3	57.48	$295\frac{1}{7}$	14	59.	$1027\frac{2}{9}$		52	8.86	
Orange	$68\frac{7}{10}$	3	29.26	$191\frac{1}{8}$	9	42.51	$1270\frac{1}{7}$	1	4	30.06	
Yellow I	$68\frac{11}{12}$	3	29.92	214	10	51.84	$1143\frac{1}{5}$		58	2.12	
Yellow-green	$61\frac{8}{11}$	3	8.02	$159\frac{1}{3}$	8	5.33	1607	1	21	34.6	
Green	$58\frac{3}{5}$	2	58.5	$163\frac{1}{16}$	8	16.8	$816\frac{5}{8}$		41	27.4	Seen first coloured prevalently as blue.
Blue-green	$58\frac{15}{22}$	2	58.74	$161\frac{11}{16}$	8	13.5	$775\frac{1}{5}$		35	31.2	" " " "
Blue	$56\frac{13}{22}$	2	56.36	$160\frac{1}{2}$	8	8.9	$399\frac{2}{5}$		20	16.6	" " " "
Violet	$58\frac{1}{20}$	2	55.8	$177\frac{3}{5}$	9	0.96	$1010\frac{1}{22}$		51	16.55	" " blue.
Grey I	$57\frac{19}{24}$	3	53.14	$130\frac{10}{11}$	6	38.75	$1399\frac{1}{2}$	1	11	2.74	At full as greyish-yellow.
Grey II	$53\frac{3}{5}$	2	43.27	$134\frac{1}{2}$	6	48.69	$1873\frac{1}{4}$	1	35	5.57	At full as greenish-grey.

Representation of the Space-Thresholds for Red Ground.

Scale : $\frac{3}{4}$ of an inch = 20 minutes.

Curves I (dotted lines) = Achromatic Threshold.
" II (lines composed of alternate dashes and dots) = Chromatic Threshold
Curve III—Characteristic C. Th. for Observer K.'s left eye (Table VIII).
" IV— " " " " right eye (Table IX).
" V— " " for 10 observers averaged (Table X).

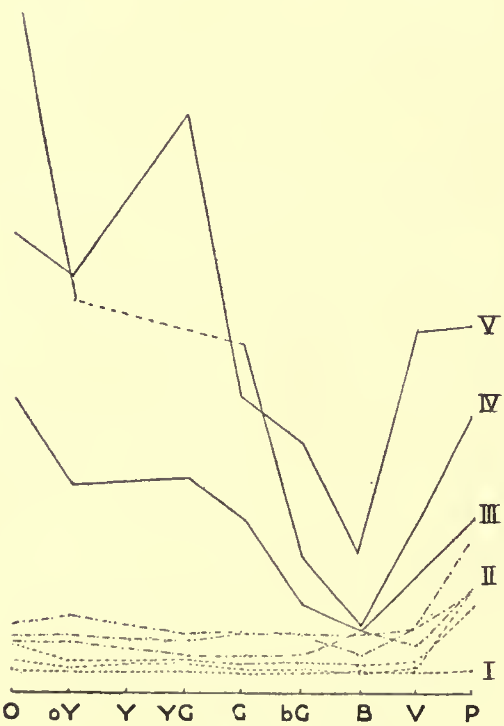

Fig. 9 (Tables VIII, IX and X).

Table XI.—On Green Ground.

Observer: Mr. Creighton.

Colour.	I. Achromatic threshold. Diagonal of diaphragm opening in 1000ths of an inch.	I. Achromatic threshold. Visual angle of diagonal of diaphragm opening. ′	″	II. Chromatic threshold. Diagonal of diaphragm opening in 1000ths of an inch.	II. Chromatic threshold. Visual angle of diagonal of diaphragm opening. ′	″	III. Characteristic colour threshold. Diagonal of diaphragm opening in 1000ths of an inch.	III. Characteristic colour threshold. Visual angle of diagonal of diaphragm opening. °	′	″	Remarks.
Purple	41	2	4.86	79	4	0.53	$227\frac{1}{5}$		11	32.04	
Red	25	1	17.36	$67\frac{1}{6}$	3	24.6	$90\frac{1}{5}$		4	34.4	
Orange	$36\frac{1}{3}$	1	50.6	$77\frac{5}{6}$	3	56.9	$310\frac{1}{2}$		15	45.	Seen purple and red at full.
Yellow I	$38\frac{3}{4}$	1	57.9	$75\frac{3}{4}$	3	50.7	$1296\frac{2}{3}$	1	5	49.54	
Yellow II	$40\frac{1}{5}$	2	5.09	$85\frac{2}{5}$	4	20.1	$530\frac{2}{3}$		26	56.4	Seen orange at full.
Yellow-green	$41\frac{3}{4}$	2	7.09	163	8	16.46	1965	1	39	45.2	
Blue-green	$65\frac{3}{5}$	3	19.8	$157\frac{1}{2}$	7	58.09	$1463\frac{1}{2}$	1	14	17.66	Seen violet-green at full.
Blue	$43\frac{4}{5}$	2	13 4	$153\frac{9}{10}$	7	48.65	$512\frac{2}{3}$		26	1.58	
Violet	$47\frac{4}{5}$	2	25.5	$73\frac{2}{5}$	3	42.9	$332\frac{1}{3}$		16	52.28	
Grey I	$36\frac{1}{5}$	1	49.9	$58\frac{4}{5}$	2	59.1	$128\frac{1}{2}$		6	31.5	
Grey II	$35\frac{3}{5}$	1	48.4	$107\frac{4}{5}$	5	28.46	$1499\frac{3}{5}$	1	16	7.6	

Table XII.—On Green Ground.

Observer: Mr. Lane.

Colour.	I. Achromatic threshold. Diagonal of diaphragm opening in 1000ths of an inch.	I. Visual angle of diagonal of diaphragm opening. ′	″	II. Chromatic threshold. Diagonal of diaphragm opening in 1000ths of an inch.	II. Visual angle of diagonal of diaphragm opening. ′	″	III. Characteristic colour threshold. Diagonal of diaphragm opening in 1000ths of an inch.	III. Visual angle of diagonal of diaphragm opening. °	′	″	Remarks
Purple	$127\frac{1}{3}$	6	27.8	$132\frac{1}{2}$	6	43.6	389		19	42.92	
Red	82	4	9.75	$93\frac{1}{3}$	4	44.1	153		7	46.	
Orange	90	4	34.1	$148\frac{2}{3}$	7	32.8	$382\frac{1}{3}$		17	40 84	Red at full.
Yellow I	$100\frac{1}{3}$	5	55.5	121	6	0.	457		23	12.06	
Yellow II	$120\frac{1}{3}$	6	6.55	$187\frac{1}{2}$	9	31.	309		15	41.24	
Yellow-green	$152\frac{1}{3}$	7	43.95	$291\frac{1}{3}$	14	47.4	1670	1	24	46.6	
Blue-green	$210\frac{1}{3}$	10	40.6	300	15	0.	1805	1	31	37.8	
Blue	$141\frac{1}{3}$	7	9 5	269	13	39.4	269		13	39.4	
Violet	129	6	33.	$148\frac{2}{3}$	7	52.8	159		8	4.3	
Grey I	$59\frac{2}{3}$	3	1.7	$165\frac{2}{3}$	8	24.55	....	....			
Grey II	$187\frac{2}{3}$	9	31.6	222	11	16.2	....	....			

Table XIII.—ON GREEN GROUND.

Observer: Dr. Kirschmann.

Colour.	I. Achromatic threshold.			II. Chromatic threshold.			III. Characteristic colour threshold.				Remarks.
	Diagonal of diaphragm opening in 1000ths of an inch.	Visual angle of diagonal of diaphragm opening.		Diagonal of diaphragm opening in 1000ths of an inch.	Visual angle of diagonal of diaphragm opening.		Diagonal of diaphragm opening in 1000ths of an inch.	Visual angle of diagonal of diaphragm opening.			
		′	″		′	″		°	′	″	
Purple	67	3	24.1	88	4	28.	628		31	42.9	Seen as reddish-purple at full.
Red	81½	4	8.24	81½	4	8.24	160		8	7.37	
Orange	80½	4	5.2	89½	4	32.6	160½		8	8.9	Seen red at full.
Yellow I	74	3	45.37	81¼	4	7.5	1860	1	34	23.36	Red-orange at full.
Yellow II	103¼	5	14.6	103¼	5	14.6	789½		40	4.8	
Yellow-green	127½	6	28.39	215	10	54.9	1027		52	8.26	
Blue-green	104	5	16.79	124	6	17.75	979½		49	43.5	
Blue	78½	3	59.	125	6	20.79	461½		23	25.72	
Violet	74½	3	46.9	99	5	1.5	362½		18	24.16	
Grey I	65	3	18.	65	3	18.	1460	1	14	7.	Orange-yellow at full.
Grey II	79	4	0.5	125¼	6	21.5	1890	1	35	56.62	Yellowish-grey at full.

Table XIV.—ON GREEN GROUND.

Observer: Mr. Shaw.

Colour.	I. Achromatic threshold.			II. Chromatic threshold.			III. Characteristic colour threshold.				Remarks.
	Diagonal of the diaphragm opening in thousandths of an inch.	Visual angle of the diagonal of the diaphragm opening.		Diagonal of the diaphragm opening in thousandths of an inch.	Visual angle of the diagonal of the diaphragm opening.		Diagonal of the diaphragm opening in thousandths of an inch.	Visual angle of the diagonal of the diaphragm opening.			
		′	″		′	″		°	′	″	
Purple	81⅔	4	8 73	112⅔	5	43.2	255⅔		12	58.73	
Red	69½	3	31.67	85	4	18 86	130⅓		6	37.	
Orange	79	4	0.53	94⅓	4	47.15	1385	1	10	19.6	
Yellow I.	82	4	9.75	90	4	34.1	332		16	51.3	
Yellow II.	90	4	34.1	107½	5	27.5	787		39	59.2	
Yellow-green	321	10	16.8	349⅔	17	45.	1339⅝	1	7	10.2	
Blue-green	205	10	24 44	260	13	11.9	1147½		58	15.2	
Blue	87½	4	26.5	228	11	34.5	303		15	22.92	
Violet	85⅓	4	19.85	108⅓	5	30.05	574⅓		29	9.4	
Grey I.	54	2	44.46	174	8	49.9	656		33	18.98	
Grey II.	133	6	45.1	256½	13	1.25	990		50	14.6	

Table X V.—On Green Ground

Observer: Mr. Dodds.

Colour.	I. Achromatic threshold.			II. Chromatic threshold.			III. Characteristic colour threshold.			Remarks.
	Diagonal of the diaphragm opening in thousandths of an inch.	Visual angle of the diagonal of the diaphragm opening.		Diagonal of the diaphragm opening in thousandths of an inch.	Visual angle of the diagonal of the diaphragm opening.		Diagonal of the diaphragm oponing in thousandths of an inch.	Visual angle of the diagonal of the diaphragm opening.		
		′	″		′	″		′	″	
Purple	$52\frac{3}{8}$	2	39.5	78	3	57.5	$95\frac{1}{8}$	4	49.66	
Red	$28\frac{1}{8}$	1	25.6	$58\frac{3}{8}$	2	57.	95	4	49.2	
Orange	$63\frac{1}{8}$	3	12.3	$73\frac{3}{8}$	3	43 5	$478\frac{1}{8}$	24	16.36	
Yellow I.	60	3	2.7	74	3	45.35	$102\frac{1}{6}$	3	11.2	Seen as orange at full opening.
Yellow II	$54\frac{5}{8}$	2	46.4	$74\frac{9}{10}$	3	48.1	$131\frac{3}{8}$	6	40.2	
Yellow-green	$156\frac{4}{5}$	7	57.53	$198\frac{1}{5}$	10	3.3	895	45	26.72	
Blue-green	91	4	37.1	$113\frac{3}{4}$	5	46.45	$354\frac{1}{2}$	17	59.8	
Blue	$53\frac{3}{4}$	2	43.7	$90\frac{1}{2}$	4	35.6	$195\frac{1}{4}$	9	54.65	
Violet	$75\frac{1}{4}$	3	49.1	$84\frac{5}{8}$	4	17.73	319	16	11.9	
Grey I.	$42\frac{1}{4}$	2	8.66	$58\frac{1}{4}$	2	57.4	....	....		
Grey II.	$70\frac{3}{4}$	3	35.46	118	5	59.4	....	....		

REPRESENTATION OF THE SPACE-THRESHOLDS FOR GREEN GROUND.

Scale : $\frac{3}{4}$ of an inch=20 minutes.

Curve	III—Characteristic	Colour	Threshold for Obs.	Dodds (Table XV).
"	IV—	"	"	"	Kirschmann (Table XIII).
"	V—	"	"	"	Shaw (Table XIV).
"	VI—	"	"	"	Lane (Table XII).
"	VII—	"	"	"	Creighton (Table XI).

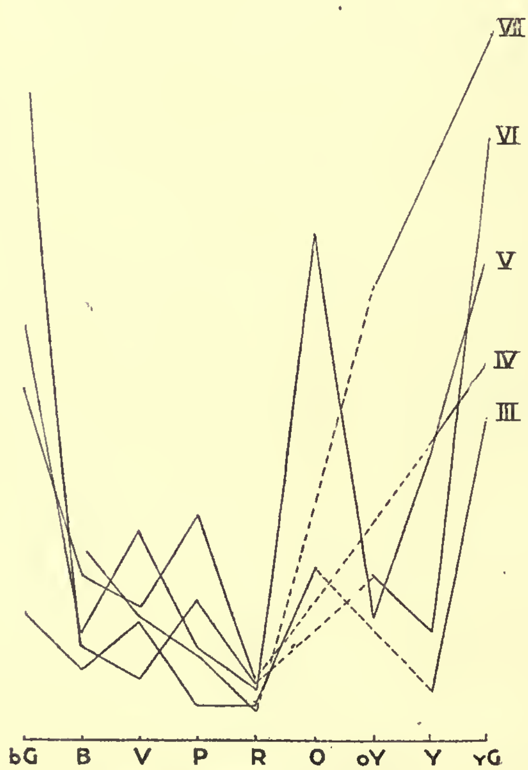

FIG. 10 (Tables XI to XV).

Table XVI.—On Blue Ground.

Observer: Mr. Robinson.

Colour.	I. Achromatic threshold. Diagonal of diaphragm opening in 1000ths of an inch.	I. Visual angle of diagonal of diaphragm opening. ′	″	II. Chromatic threshold. Diagonal of diaphragm opening in 1000ths of an inch.	II. Visual angle of diagonal of diaphragm opening. ′	″	III. Characteristic colour threshold. Diagonal of diaphragm opening in 1000ths of an inch.	III. Visual angle of diagonal of diaphragm opening. °	′	″	Remarks.
Purple......	75 1/6	3	48.94	128 1/2	6	31.52	368 5/6		18	43.46	Seen first coloured red.
Red........	59 1/6	3	0.22	81 1/8	4	7.	81 1/8		4	7.	“ “
Orange.....	74 1/2	3	16.5	116 1/3	5	54.34	....	...	...	...	“ “ and red at full open-[ing
Yellow I....	55 1/3	2	48.54	92 2/3	4	42.26	(2041 2/3)	(1	43	38.6)	Seen first coloured red; orange at full.
Yellow II...	57 7/12	2	55.4	156 11/12	7	57.96	(1635)	(1	23	0.)	“ “ chamois at full.
Yellow-green	65	3	18.	256 1/5	13	0.33	1294 4/5	1	6	44.	
Green......	88	4	28.	367	18	43.	(1619 2/3)	(1	22	13.28)	
Blue-green..	86 5/6	4	24.48	271 11/12	13	48.26	(2066 1/3)	(1	44	53.58)	Seen at full plain green.
Violet......	82 5/6	4	49.33	134 5/12	6	49.5	830 1/2		42	3.88	
Grey I......	49 1/12	2	29.51	145 1/12	7	21.92	(2140 3/5)	(1	48	39.74)	Seen first coloured red; at full a grey yellow.
Grey II.....	74 1/6	3	45.91	213 1/2	10	50.32	....	...	...	...	Seen first coloured red.

Table XVII.—On Blue Ground.

Observer: Mr. Dodds.

Colour.	I. Achromatic threshold.			II. Chromatic threshold.			III. Characteristic colour threshold.				Remarks.
	Diagonal of diaphragm opening in 1000ths of an inch.	Visual angle of the diagonal of diaphragm opening.		Diagonal of diaphragm opening in 1000ths of an inch.	Visual angle of diagonal of diaphragm opening.		Diagonal of diaphragm opening in 1000ths of an inch.	Visual angle of diagonal of diaphragm opening.			
		′	″		′	″		°	′	″	
Purple......	54⅓	2	46.	64½	3	16.5	104¾		5	19.	Seen first coloured red.
Red........	47⅔	2	25.	61	3	5.8	66½		3	22.5	" "
Orange.....	65⅔	3	20.	89⅓	4	32.	(960⅔)		(48	46.)	" " and red at full opening.
Yellow I....	67⅔	3	26.2	101⅓	5	8.66	1649	1	23	42.6	" " and orange-red at full.
Yellow II...	53⅓	2	42.4	88⅓	4	29.	535		27	9.2	" "
Yellow-green	73⅚	3	44.9	167¾	8	30.9	498¼		25	19.66	
Green......	102	5	10.68	173⅓	8	48.4	548¼		27	50.	
Blue-green..	108⅚	5	31.5	194⅚	9	53.4	826½		41	57.4	
Violet......	82⅚	4	2.3	91⅙	4	37.6	(105⅙)		(5	20.3)	Seen first coloured red; at full purple.
Grey I......	40	2	1.8	90	4	34.1	....	....	....	....	" "
Grey II.....	75⅔	3	50.16	158	8	1.2	....	....	....	....	

Table XVIII.—On Blue Ground.

Observer: Dr. Kirschmann (Right Eye.)

Colour.	I. Achromatic threshold. Diagonal of diaphragm opening in 1000ths of an inch.	I. Visual angle of diameter of diaphragm opening. ′ ″	I. Visual angle of diagonal of diaphragm opening. ′ ″	II. Chromatic threshold. Diagonal of diaphragm opening in 1000ths of an inch.	II. Visual angle of diameter of diaphragm opening. ′ ″	II. Visual angle of diagonal of diaphragm opening. ′ ″	III. Characteristic colour threshold. Diagonal of diaphragm opening in 1000ths of an inch.	III. Visual angle of diameter of diaphragm opening. ′ ″	III. Visual angle of diagonal of diaphragm opening. ° ′ ″	Remarks.
Purple......	100½	3 36.46	5 6 12	146	5 14.46	7 24.72	397¾	14 16.68	20 11.54	Seen first coloured red.
Red........	71¾	2 34.54	3 38.55	84½	3 2.	4 17.38	134½	4 49.69	6 49.68	" "
Orange.....	85¼	3 3.6	4 19.66	105	3 46.14	5 19 8	229¾	8 14.8	11 39.82	Seen at full open'g as red.
Yellow I....	68	2 26.46	3 27.12	100	3 35.4	5 4.62	1241¾	44 39.98	1 3 9.8	Seen first coloured red.
Yellow II...	100½	3 36.46	5 6.12	126½	4 32.46	6 25.32	1119	40 10.14	56 48.4	
Yellow-green	122½	4 23.84	6 13.13	210½	7 33.38	10 41.18	801	28 45.22	40 39.82	Seen first coloured red.
Green......	124¾	4 28.68	6 20.	205	7 21.54	10 24.44	570½	20 28.76	28 57.74	" " "
Blue-green .	187¼	6 43.80	9 30.36	272¼	9 46.38	13 49.27	1944¼	55 26.28	1 18 23.96	" " brown.
Violet......	101¾	3 39.14	5 9.92	131¼	4 42.68	6 39.8	1635¾	58 43.04	1 23 2.26	" " red.
Grey I......	106¼	3 48.84	5 23.64	92¼	3 18.69	4 40.89	(1588½)	(57 1.38)	(1 20 38.36)	Seen orange-yellow at full.
Grey II.....	128½	4 36.77	6 31.41	190¼	6 49.76	9 39.50	877	31 28.91	44 31.31	Seen dirty yellow at full.

Table XIX.—On Blue Ground.

Observer: Dr. Kirschmann (Left Eye).

Colour.	I. Achromatic threshold.		II. Chromatic threshold.		III. Characteristic colour threshold.		Remarks.
	Diagonal of diaphragm opening in 1000ths of an inch.	Visual angle of diagonal of diaphragm opening.	Diagonal of diaphragm opening in 1000ths of an inch.	Visual angle of diagonal of diaphragm opening.	Diagonal of the diaphragm opening in 1000ths of an inch.	Visual angle of diagonal of diaphragm opening.	
		′ ″		′ ″		° ′ ″	
Purple	99	5 1.5	139	7 3.4	462	23 27.2	Seen first coloured red.
Red	56	2 50 55	95	4 49.2	95	4 49.2	
Orange	78	3 57.5	136	6 54.3	2250	1 54 13.	Seen first coloured red and remains red to [the end.
Yellow I	70	3 33.2	101	5 7.6	1098	55 40.7	Seen first coloured red.
Yellow II...	90	4 34.1	160	8 7.33	1400	1 11 4.36	" " "
Yellow-green	138	7 0.4	205	10 24.45	1139	57 49.4	" " brown.
Green	125	6 29.9	190	9 38.66	1970	1 40 0.36	" " reddish brown.
Blue-green ..	289	14 40.32	305	15 29.04	822	41 43.84	
Violet	100	5 4.55	170	8 37.8	....	....	At full opening only violet purple.
Grey I......	65	3 18.	117	5 56.3	1682	1 25 23.33	
Grey II	82	4 9.75	222	11 16.2	1098	55 44.52	

Table XX.—On Blue Ground.

Mixed Observers (8).

Colour.	I. Achromatic threshold.			II. Chromatic threshold.			III. Characteristic colour threshold.				Remarks.
	Diagonal of the diaphragm opening in 1000ths of an inch.	Visual angle of the diagonal of diaphragm opening.		Diagonal of the diaphragm opening in 1000ths of an inch.	Visual angle of the diagonal of diaphragm opening.		Diagonal of diaphragm opening in 1000ths of an inch.	Visual angle of the diagonal of diaphragm opening.			
		′	″		′	″		°	′	″	
Purple......	$74\frac{1}{2}$	3	46.9	$101\frac{1}{2}$	5	9.2	$660\frac{1}{4}$		33	31.11	Seen first coloured red.
Red	$54\frac{5}{8}$	2	46.01	$67\frac{5}{7}$	3	26.21	$121\frac{7}{8}$		6	11.23	" " "
Orange	55	2	47.49	$96\frac{1}{8}$	4	52.8	$607\frac{5}{14}$		30	50.	" " "
Yellow I....	$51\frac{3}{8}$	2	36.49	$67\frac{1}{7}$	3	24.51	1514	1	16	51.46	" " "
Yellow II ..	$70\frac{13}{18}$	3	: 5.42	$122\frac{1}{7}$	6	12.	$1159\frac{6}{7}$		58	39.77	" " "
Yellow-green	$60\frac{3}{7}$	3	31.48	$236\frac{1}{5}$	11	59.4	$1110\frac{2}{3}$		56	23.	
Green	$114\frac{9}{14}$	4	43.93	$239\frac{5}{14}$	12	9.11	$1250\frac{2}{3}$	1	3	29.42	Seen first coloured green or blue.
Blue-green ..	$99\frac{7}{8}$	5	4.22	$316\frac{3}{8}$	16	3.67	$1933\frac{3}{4}$	1	38	9.81	
Violet	$71\frac{1}{6}$	3	36.7	112	5	41.15	$1010\frac{3}{4}$		51	18.6	Seen first coloured red.
Grey I......	$42\frac{1}{2}$	2	9.45	92	4	40.23	$1100\frac{1}{10}$		55	50.84	" " "
Grey II	83	4	12.82	212	10	45.75	....		....	....	

Representation of the Space-Thresholds for Blue Ground.

Scale : ¾ of an inch=20 minutes.

Curve I—Achromatic Threshold (average of 8 observers).
" II—Chromatic " "
" III—Characteristic C. Thr. for Obs. Dodds (Table XVII).
" IV— " " " Kirschmann, left eye (Table XIX).
" V— " " " " right eye (Table XVIII).
" VI— " " 8 observers, averaged (Table XX).

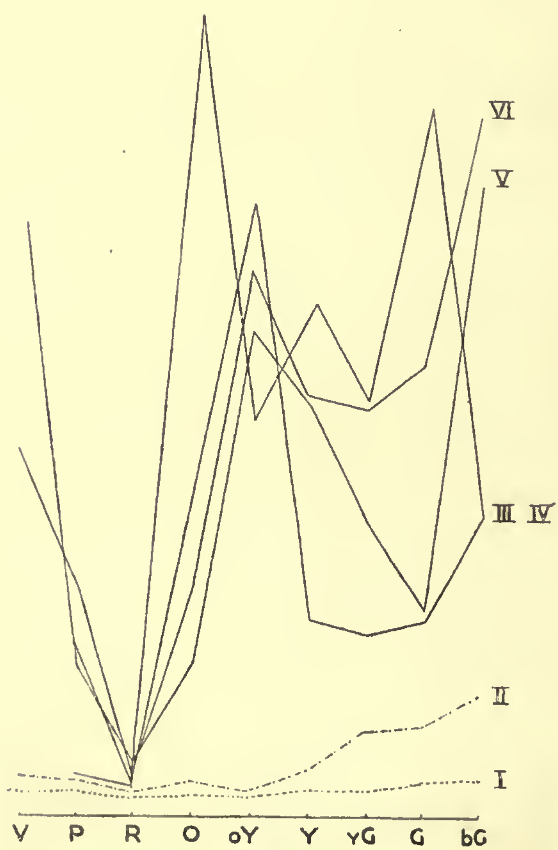

Fig. 11 (Tables XVI to XX).

SUMMARY.

I.—Black Ground.

1. The chromatic space-threshold has two decided maxima, in the yellow and in the blue regions.

2. The characteristic colour threshold has three strongly marked maxima, at orange, yellow-green and violet; and three decided minima, in the yellow, blue-green and red-purple regions.

3. For red the achromatic, chromatic and characteristic thresholds coincide; and for yellow the chromatic and characteristic thresholds coincide.

II.—Grey Ground.

1. Similar results to those noted for black ground are obtained for the maxima and minima, except that the second minimal region includes yellow no. 1 (orange-yellow), and that the second maximal region includes green. We may also notice that the maxima are considerably raised while the minima retain in general their former position.

III.—Coloured Grounds.

A very peculiar change of the antagonistic colour relations seems to take place. In small areas it is not the complementary colour which finds its most favourable conditions for being recognized on coloured grounds. On a red ground the minimum of the characteristic colour threshold is decidedly in the blue; and on a blue ground the curve has an exceedingly marked minimum in the red. Thus for small surfaces on coloured grounds blue and red act as complementaries.

A CASE OF ABNORMAL COLOUR-SENSE EXAMINED WITH SPECIAL REFERENCE TO THE SPACE-THRESHOLD OF COLOURS

By

J. W. BAIRD, B.A., and R. J. RICHARDSON, B.A.

A CASE OF ABNORMAL COLOUR SENSE, EXAMINED WITH SPECIAL REFERENCE TO THE SPACE THRESHOLD OF COLOUR.

BY J. W. BAIRD, B.A. AND R. J. RICHARDSON, B.A.

Notwithstanding the extensive literature already before the world upon the subject of colour-blindness, we hope that an account of our examination of the case about to be described may be of interest. The classification of the human family into colour-blind and non-colour-blind is at best a matter of rough and somewhat arbitrary judgment. The colour sense is called normal when expressions and actions lead us to suppose that the relations of the colour sensations are the same as with the majority of people, while persons whose expressions and actions show us that these relations differ considerably from those of the majority are called colour-blind, or persons of abnormal colour-sense.*

Colour-blind persons may, according to Dr. Kirschmann, be classified on the basis of the degree of their abnormality into (1) achromates—who are totally devoid of colour-sense and see objects only in various degrees of grey, (2) dichromates—who see only two antagonistic colour qualities, such as red and green or blue and yellow, and (3) abnormal polychromates—who distinguish more than two colours, though the relations of their colour-sensations differ from those of the normal.

The results of the investigations of colour-blindness can only approximate to accuracy, owing chiefly to the difficulties which we always encounter when we attempt to ascertain and describe the psychic states of others. Colour-blind individuals use the ordinary colour vocabulary, which is quite inadequate to express their sensations of colour.

The experiments which furnished the data for this paper were conducted in the psychological laboratory of the University of Toronto at the request of Dr. Kirschmann, director of the laboratory, to whom our thanks are due for co-operation and suggestions.

*This is the definition of normal colour sense and colour-blindness which Dr. Kirschmann gives in his lectures on Psychological Optics.

We are also indebted to Mr. W. B. Lane, M.A., now Fellow in Psychology in the University of Wisconsin, to Mr. F. S. Wrinch, M.A. and Mr. A. H. Abbott, B.A., Assistant in the psychological laboratory, for valuable assistance in our work.

This case of colour-blindness was investigated in the following ways:

I. Spectroscopic examination.

II. Stilling's Pseudo-isochromatic tables.

III. Colour equations.

IV. Experiments on space-threshold of colours under contrast influence.

I. A series of observations was made upon the solar spectrum by observer R. (whose colour-sense is abnormal), with a view to determining the nature of his deviation from the normal. The wave-lengths are calculated by graphical interpolation, the numbers for the principal lines being taken from the tables of Rowland. Mr. R. always claims that he sees three distinct colours, red, green and blue, in the spectrum.

Using the left eye Mr. R. saw the light,

920 $\mu\mu$ to 670 $\mu\mu$, dark brownish red
670 $\mu\mu$ to 540 $\mu\mu$, uniform red
(590 $\mu\mu$, red of deepest saturation)
540 $\mu\mu$ to 493 $\mu\mu$, green
(513 $\mu\mu$, best green)
493 $\mu\mu$ to 384 $\mu\mu$, blue
(429 $\mu\mu$, deepest blue).

The maximum of light intensity was seen at 590 $\mu\mu$. At 762 $\mu\mu$. the line A was seen as distinctly as the other lines, whilst observers B and K could see it, but with difficulty. In the red end of the spectrum in addition to the lines A, B, and C, other lines were clearly distinguishable at points corresponding to the following wave-lengths: 603 $\mu\mu$, 626 $\mu\mu$, 644 $\mu\mu$, 728 $\mu\mu$, *and at two points beyond the A line in the ultra-red spectrum.* The wave-lengths corresponding to the latter points are not computable from any data within our reach, but we estimate them from our interpolation-curve to be the Z line and the X_4 line (Abney), corresponding to which are the wave-lengths 822 $\mu\mu$ and 880 $\mu\mu$ respectively.

For the right eye the colours and their positions were given as follows:—

900 $\mu\mu$ to 687 $\mu\mu$, dark brownish red
(726 $\mu\mu$, most characteristic brown-red
687 $\mu\mu$ to 541 $\mu\mu$, red
(590 $\mu\mu$, red of deepest saturation)

541 $\mu\mu$ to 501 $\mu\mu$, green
(527 $\mu\mu$, best green)
501 $\mu\mu$ to 384 $\mu\mu$, blue
(427 $\mu\mu$, blue of deepest saturation).

The lines in the ultra-red spectrum (especially that at 822 $\mu\mu$) were seen as distinctly as with the left eye, whilst observers B and K could see absolutely nothing beyond 762 $\mu\mu$ and 770 $\mu\mu$ respectively.

In addition to the examination of the solar spectrum with the spectroscope, observations were made upon an inverted spectrum* projected on a screen by means of an arrangement recently devised by Dr. Kirschmann for the purpose of comparing the ordinary and the inverted spectrum. One of these spectra had its colours arranged in the usual order—purple being absent. The other was thrown upon the screen parallel with and contiguous to the first, but with the order of the colours inverted; that is, the colours proceeded through yellow, orange, red, purple and violet to blue—green being absent. A photograph of the two spectra is given (p 101.)

The only difference noted by observer R. in these spectra was that in the inverted form the dark brownish red was missing. *He could distinguish no difference in colour tone in the central section of the two spectra*, that is, between the purple of the inverted and the green of the ordinary spectrum. The statements of observer R. were essentially the same when the two spectra were divided by means of interference bands.

II.—A series of tests for colour-blindness by means of Stilling's Pseudo-Isochromatic Tables (third edition, 1889) yielded the following results:

Plate I (orange-red figures on brown ground) was read slowly and with a great deal of uncertainty. The ground was called green, the darker patches brown or purplish. The figures were called blood-red, their lighter patches yellowish. The figures were more easily distinguished through a red glass, but best of all through blue.

Plate II (orange-red figures on brown ground) was more easily read than Plate I, but also with considerable uncertainty. The ground was called green, the darker patches murky red like the extreme red end of the spectrum. The figures were said to be com-

*For description of the inverted spectrum see the article by Dr. Kirschmann on Colour Saturation in the American Journal of Psychology, Vól. vii., p. 387, and note at the end of these articles.

posed of patches of red and orange-red. Green glass was of no help in reading the figures, but violet glass was of great assistance.

Plate III (orange figures on brown ground) was read with much hesitation. The figures were thought to be a poorly saturated red and the ground to be of patches of light and dark green. It was easily read with a blue glass.

Plate IV (red figures on coffee-brown ground) was more easily read than plates I, II or III. The figures were seen as bright blood-red, the ground in two saturations of purple. It was less easily read through blue and green glasses, more easily through red glass.

In plate V (wine-red figures on dark chocolate ground) the figures could be seen. The colours of the plate seemed to be two saturations of purple, the darker patches like clotted blood, the lighter like "purple-green." Through a red glass the figures could be read, though with much hesitation.

Plate VI (green figures on brown ground) was read very slowly, and only after a deliberate examination. It could not be read through blue glass; yellow-green glass improved it, though it was not yet distinct. Through red glass it was quite distinct and easily read. The ground was said to be bright reddish purple, the figures purplish red.

Plate VII (cherry-coloured figures on greyish ground) was fairly well read. The ground had lighter patches of poorly saturated purple or well saturated green, and darker patches of dark brown or dark purple. The figures were quite distinct through a red glass. Blue and green glasses were of no help. The purple glass was an improvement but not so much so as the red.

Plate VIII (red figures on orange ground) was more easily read than any of the above. The figures were "purplish green," the ground composed of patches of blood-red and poorly saturated red. Red, green, blue and purple glasses were of no assistance.

Plate IX (lilac figures on grey-green ground) was read very slowly, one figure at a time. The figures were blue-green, the ground green and brown-green or purple. None of the glasses were of any assistance.

Plate X (red figures on light brown ground) was the most distinct of all. The figures were a very well saturated red, the ground a poorly saturated green with a less saturated red than the figures. Red glass altogether prevented the possibility of reading it, green and blue glasses made no difference in the ease with which it could be read.

III.—Colour equations by means of rotating discs were also made, of which we give the following examples:

I. 286° green + 74° blue = 130° white + 230° black.

II. 68° blue + 292° red = 42° white + 380° black.

In the first of these cases the whole disc was seen as grey, and in the second it was described as purple.

IV.—For a description of the apparatus used in the contrast experiments the reader is referred to the paper by Mr. W. B. Lane. The apparatus was arranged in such a way that it was possible to measure with great accuracy the magnitude which is necessary for a coloured surface, in order to be seen (*a*) as light, (*b*) as colour, (*c*) in its proper colour. The experiments can be performed for a black or any uncoloured ground as well as under the influence of pure colour contrast, *i.e.* for coloured grounds under approximately complete exclusion of intensity contrast and saturation contrast.

In the following tables which are compiled from records of experiments made by means of this apparatus, we shall compare the abnormal colour sense of observer R. with the normal, by a statement of the visual angles which represent their achromatic and chromatic thresholds. By achromatic threshold we understand the smallest visual angle at which the coloured surface concerned could be seen as something different from the ground; by chromatic threshold, the smallest visual angle at which the colour of the surface was identified.

The inducing surface for Table I was grey, into the centre of which were introduced in turn the ten different colours which were at our disposal. The colours were almost pure spectral colours; their spectroscopic analysis also will be found in the foregoing paper by Mr. Lane, who used for the purpose of his research the same apparatus.

Table I.—Inducing Surface Grey.

Induced colour.	Size of visual angle at which the induced colour was seen as something different from the ground.		Size of visual angle when the induced colour was identified.	
	By normal colour sense.	By abnormal colour sense.	By normal colour sense.	By abnormal colour sense.
	′ ″	′ ″	′ ″	
Red	2 56.6	3 45.4	6 36.7	*Not identified.
Orange	3 33.2	5 50.2	4 55.5	"
Orange-yellow	4 3.7	6 14.7	9 43.7	"
Yellow.......	3 35.45	4 34.1	4 47.67	18′ 59.2″
Yellow-green .	3 39.3	5 14.7	15 16.8	32′ 43.32″
Green	3 43.2	7 33.9	16 25.4	Not identified.
Blue-green ..	3 33.2	7 27.7	8 9.85	"
Blue	4 25.9	7 39.1	6 50.4	9 58″
Violet	4 12.8	7 4	23 36.4	15′ 9 6″
Purple	4 17.4	4 46.2	8 23.55	1° 7′ 6.6″

*The full opening of the induced surface subtended a visual angle of 2° 6′ 38.92″, beyond which magnitude the experiment could not be extended, and below it the observer was unable to identify the induced colour in its own quality. These cases are indicated in our tables by "not identified."

Table II.—Inducing Colour Red.

Induced colour.	Size of visual angle at which the induced colour was seen as something different from the ground.		Size of visual angle when the induced colour was identified.	
	By normal colour sense.	By abnormal colour sense.	By normal colour sense.	By abnormal colour sense.
	′ ″	′ ″	° ′ ″	
Orange	3 16.05	7 35.4	1 4 80.06	Not identified.
Orange-yellow	3 29.92	7 54.1	1 14 35.02	"
Yellow	4 27.3	7 4	Not identified.	1° 17′ 40.8″
Yellow-green .	3 8.02	5 12.2	1 21 34.6	54′ 39 4″
Green	2 58.5	5 47 3	41 27.4	Not identified.
Blue-green ...	2 58.74	5 48	46 7.11	11′ 48.2″
Blue	2 56.36	6 1	26 26.4	15′ 9.26″
Violet	2 48.5	5 56.3	51 16.55	Not identified.
Purple	3 57.48	6 16.2	52 8.86	37′ 38″

Table III.—Inducing Colour Green.

Average of five trials.

Induced colour.	Size of visual angle at which the induced colour was seen as something different from the ground.		Size of visual angle when the induced colour was identified.	
	By normal colour sense.	By abnormal colour sense.	By normal colour sense.	By abnormal colour sense.
	′ ″	′ ″		° ′ ″
Red	3 24.1	6 11.6	4′ 43.2″	1 25 18.3
Orange	4 37.1	9 38.6	Not identified.	1 15 41.6
Yellow	6 5.6	6 30	15′ 41.2″	Not identified.
Yellow-green .	6 17.7	8 7.5	Not identified.	15 1.8
Blue-green ...	9 11.3	6 23.8	19′ 15.98″	21 38.4
Blue	5 7.6	8 4.3	13′ 33.4″	15 5.8
Violet	6 36	2 52.9	10′ 48.8″	35 26.8
Purple.......	6 30	11 11	19′ 45″	1 8 1.5

Table IV.—Inducing Colour Blue.

Average of three trials.

Induced colour.	Size of visual angle when induced colour was seen as something different from the ground.		Size of visual angle when induced colour was identified.	
	By normal colour sense.	By abnormal colour sense.	By normal colour sense.	By abnormal colour sense.
	′ ″	′ ″	° ′ ″	′ ″
Red	3 38.55	4 28	6 49.68	52 48.8
Orange	4 19.66	4 58.5	11 39.82	Not identified.
Yellow	5 6.12	3 39.3	56 48.4	"
Yellow-green .	6 13.13	9 5.2	40 39.82	"
Green	6 20	15 26.3	28 57.74	"
Blue-green ...	9 30.36	7 33.8	1 18 23.96	"
Violet	5 9.92	7 6.4	1 23 2.26	"
Purple	5 6.12	10 58	20 11.54	"

SUMMARY.

1. In the case of abnormal colour-sense, which we have examined, the colour spectrum is not only not shortened, but it is actually *considerably lengthened* at the red end. The violet end is seen as by the normal eye.

2. Only three qualities were distinguished in the spectrum—red, green, and blue,—and it remains uncertain whether green was identical with grey or not.

3. The differences in the blue part of the spectrum were claimed to be but differences of saturation.

4. The only difference detected between the ordinary and the inverted spectrum was that the deepest red was missing from the inverted spectrum; otherwise the two were identical—*i.e.*, the ordinary spectrum contained the same colours as the inverted and in the same order, except red. Nothing was missing from the ordinary spectrum which was found in the inverted spectrum. A point in the inverted spectrum—purple—was judged to be exactly like the green of the ordinary spectrum which stood below it.

5. As to the experiments on the space threshold:

a. In the case of the grey background [non-contrast] the variations of the achromatic threshold for the normal and abnormal were fairly parallel for purple, red, orange, and yellow; but the threshold for the colour-blind observer was considerably higher throughout.

b. A similar correspondence was noticeable in the chromatic threshold on red ground.

(In both of the above the achromatic threshold was invariably higher to the abnormal than to the normal colour sense.)

c. When green was the inducing colour the achromatic threshold for the abnormal colour-sense was considerably lower than that for the normal in the cases of blue-green, and violet, though higher for all other colours.

d. In the blue contrast the normal threshold gradually increased from red to blue-green and decreased from violet to red while the abnormal had its maximum points in green and purple, and its minimum in yellow.

e. With regard to the chromatic [characteristic colour] threshold, the fact that only the yellow and blue, and sometimes the neighbouring tones were identified seems to prove clearly the dichromatic character of this case of colour-blindness.

6. The influence of border contrast was distinctly noticeable in the abnormal colour-sense, usually appearing when the visual angle reached a magnitude of about 1° 4′, and persisting to the full opening.

7. The effect of contrast was usually much greater in the abnormal sense than in the normal for those colours which were at the disposal of the colour-blind observer.

The accompanying diagram gives a graphical representation of the achromatic threshold for the normal and the colour-blind eye.

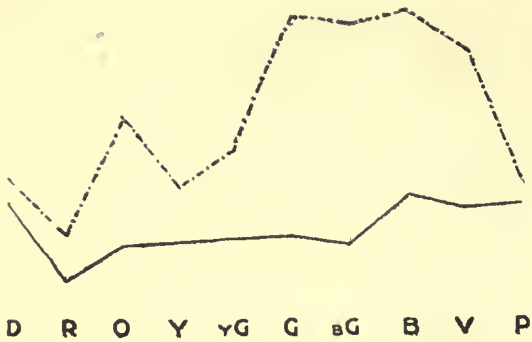

Fig. 1.

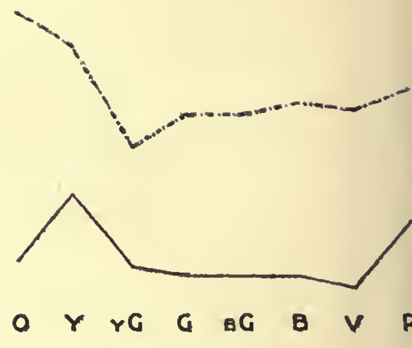

Fig. 2.

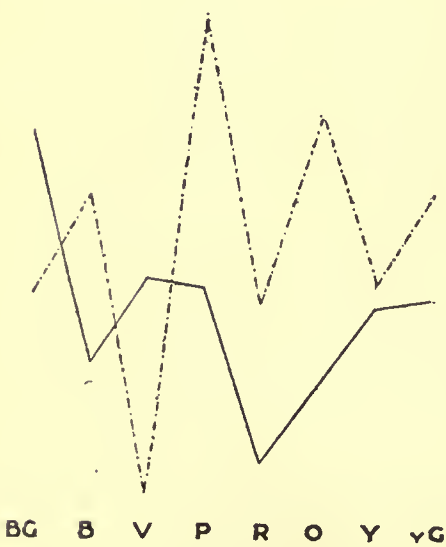

Fig. 3.

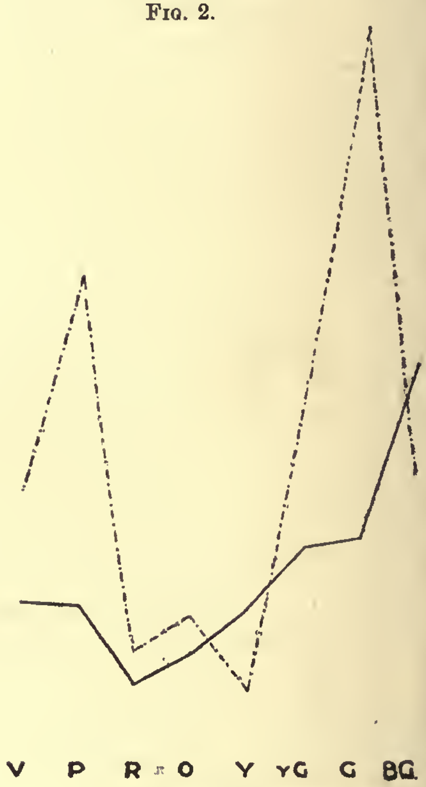

Fig. 4.

Achromatic Threshold for Normal Colour-Sense and Colour-Blind.

Fig. 1. Grey Ground (Table I).
" 3. Green " (Table III).

Fig. 2. Red Ground (Table II).
" 4. Blue " (Table IV).

Curve ——— = Normal Colour-Sense.
" ------- = Colour-Blind.

ADDITIONAL REMARKS ON COLOUR BLINDNESS.

BY A. KIRSCHMANN.

The case of colour-blindness above reported deserves attention in several respects. Its most particular feature is the fact that in this case of abnormal colour-sense we have to deal *with an enlarged spectrum.* A considerable part of the ultra-red spectrum, which is invisible for the normal eye, is seen and lines in it are distinguished. I have always laid emphasis upon the fact that the length of the spectrum and the manifoldness of colour-sensations do not stand in any direct connection. There are cases in which the spectrum is shortened and the colour-sense is quite normal; there are others of partial or total colour-blindness in which the spectrum has its ordinary extension on both sides. The case in question in the foregoing article *proves that pronounced colour-blindness may even be associated with a considerably lengthened spectrum.*

Further, the experiments with the inverted spectrum prove beyond any possible doubt:

1. That below about $540\mu\mu$ no differences in colour quality are distinguished. Red, orange and yellow are seen as the same colour-tone. It must be pointed out that from the circumstance that all these colours are called *red,* it does not follow that the colour-blind sees them like our red.

2. All wave-lengths above about $500\text{-}490\mu\mu$ are again seen as one quality (which is called blue), but in different saturations.

3. The colour missing in the ordinary spectrum, reddish purple, *is seen exactly of the same quality as that in the middle of the spectrum,* the green between E and F. Whether this part of the spectrum is seen identical with colourless light or not cannot be decided with certainty from the results of the observations. The fact that the colour-blind localizes the "best green," and that he decidedly denies that it could be called white or grey seem to contradict this; the blunders in the distinction of grey and green pigments, on the other hand, support the purely dichromatic character of this system of colour-sensations.

I may mention in this connection that so-called colour-equations are not an absolutely infallible means upon which to decide regarding dichromacy in cases similar to the one stated above. The assumption that everything which is colourless for the normal eye must appear so to the colour-blind also, is incorrect as soon as it is admitted that the relations of complementary qualities may be shifted in the case of an abnormal colour-sense.

The case above reported is different from all other cases which have come under my notice, and thus forms one proof more for the untenableness of those theories which try to force all cases into two classes, with little modifications—red-green blinds and blue-yellow blinds. On account of the enlarged spectrum, which had not been observed hitherto, this case may find its place at the side of others which give a good deal of trouble to the adherents of the component theories, that of Von Vintschgau,* and case five in my own paper.† (I have just learned that the latter, a very interesting case of monocular colour-blindness, has been investigated again by Hering, and I sincerely trust that this renowned physiologist will no longer hesitate to publish the results of his investigation, which adherents and opponents of the component theories are equally anxious to read.)

It is not seldom that colour-blinds ask whether their defect could be cured or not; or they ask for the prescription of glasses with which "they can see the colours." Without entering into any discussion of the question whether the colour-sense might be changed or not by means of drugs, hypnotism, etc., I may be permitted to state, that it is quite possible to furnish the colour-blind with some means by which roughly to avoid mistakes in the choice and designation of colours. This would be of great value to him, especially where his occupation involves a constant dealing with pigments or other colours. I have twice given "glasses" to colour-blinds. The principle itself is not new, for I think Delboeuf‡ used many years ago a solution of fuchsin for similar purposes. Red objects look bright when seen through this medium, whilst green surfaces appear dark. Thus a red-green blind may distinguish red and green, by judging of their brightness when seen through a medium which absorbs either all the red or all the green rays. The first of these two cases was a Mrs. A., a milliner in San Francisco. She was a dichromate with the indifference-line near *B*. Her defective colour-sense did not seem to have any damaging influence on her business; it obviously caused more trouble to her own conscience than to the taste of her customers.

*Pflügers Archiv, xlviii, p. 431 ff.

†Philosoph. Studien, viii, p. 196 ff.

‡ I have not the literature at hand.

With respect to the question of the heredity of colour-blindness it may be worth while to mention that a sister of this lady was colour-blind also. Her brother was not colour-blind. Whether one of her parents was colour-blind or not could not be ascertained. Of the two children of Mrs. A., a daughter of thirteen years was not colour-blind, whilst the little son, ten years old, had the same defect in his colour-sense as his mother.

I gave to this lady two "glasses," *i.e.*, two combinations of plates of coloured glass and gelatine films, the composition and absorbing power of which, as approximately ascertained with the spectroscope, were as follows:

	Light transmitted with small opening of the slit.	Light transmitted with wide opening of the slit.
I. Composed of 1 red glass (copper-oxide), 1 film of yellow gelatine, 1 film of purple gelatine	700—590$\mu\mu$	750—570$\mu\mu$
II. Composed of several blue and green gelatine films	550—480 "	560—460 "

Combination I bore on its handle the direction: All objects which, viewed through this glass, lose much of their brightness, *cannot be red.* Combination II had the direction: All surfaces which lose much of their brightness, when looked at through this glass, *cannot be green.* Now, in order to become accustomed to the use of these instruments, and to use them with success in cases of lighter saturation-degrees of the colours, the colour-blind must practise with samples which form a kind of standard with which to compare the surfaces in question. For this purpose I gave the colour-blind two sets of samples, one on black, the other on white ground. Each of the two sets consisted of thirty-six little discs of coloured paper, about an inch in diameter. Ten of these discs represented the spectral colours in as good a saturation as pigment papers allow, ten the corresponding lighter tints, and ten others the darker shades of the same colours. Besides these there were three samples of grey and three of brown. The names of the colours were written (with gold bronze, in order to be visible through any glass) at the side of each disc.

If the colour-blind is uncertain whether to call a surface red, green, or grey, he will look at it through the glasses described and in more difficult cases of slighter tinges of these colours he will carefully compare the behaviour of their intensities under the influence of the absorbing media with that of the samples. It is comparatively easy to identify grey surfaces by means of this method, for they keep up about the same brightness for both absorbing glasses, whilst any red or green tinge reveals itself by contrary behaviour toward the two instruments. The distinction of very whitish colours, as light rose, lilac, etc., requires some practice, and a sharp and unprejudiced judgment.

The other colour-blind to whom I gave "glasses" of similar construction was Mr. S., a student at the School of Practical Science in Toronto. He is a pronounced dichromate, with an indifferent region extending a little on both sides of the line F. I made in this case three combinations, one for the red end of the spectrum to about $620\mu\mu$, one which absorbed everything except the green, and a third which absorbed the green and transmitted the rays of the two ends of the spectrum fully. As standard colours I gave in this case three sets, one on white, one on black, and one on grey ground. Each set contained twenty colours, each in three degrees of saturation. There was a special table arranged for grey and brown. The successful use of so complicated an arrangement requires of course on the part of the colour-blind intelligence, sharp observation, and perseverance.

The accompanying plate shows, as well as it can be reproduced by a half-tone cut, in its upper part (Fig. 1) a photograph of the spectra mentioned in the foregoing article. The limits of the visible spectra are indicated by perpendicular lines crossing the whole field. At the end of the shorter waves the photograph shows distinctly beyond the visible part the ultra-violet in the ordinary, and the ultra-yellow in the inverted spectrum. The two smaller photographs, Figs. 2 and 3, are obtained by means of light reflected from very thin films of mica. On account of the path-difference of the rays from the front and the back surface of the film they show interference-bands, which are equally visible in the ordinary as in the inverted spectrum.*

*As no description has yet been given of the apparatus by means of which the normal and inverted spectra are projected on the screen together, I may here give a brief account of it. I have always used an electric lantern as the source of light in this experiment, since it is more convenient than sunlight. With the exception of the plate to be next described, the rest of the apparatus consists of a large lens and a prism. The special part of the apparatus consists of a very thin plate of glass (I have used a microscope cover-glass) of any desired size, half of which is made opaque with the exception of a slit of the required width and length. The other half of the glass plate is left transparent with the exception of a portion, exactly opposite the end of the above mentioned slit, and about the same size, which is also made opaque. This opaque strip I have called a "negative slit." The plate is inserted in the path of the rays of light, which after passing it are focussed and deflected by the lens and prism respectively.

Fig. 1.

Fig. 2.

Fig. 3.

A CONTRIBUTION
TO THE PSYCHOLOGY OF TIME

By

M. A. SHAW, B.A., and F. S. WRINCH, M.A.

CONTENTS.

I. Historical sketch of the problem 1

II. Theory of time estimation 17

III. Experiments on the effect of a lapse of time between the normal and comparative intervals.................. 25

IV. Weber's law and time estimation 30

V. Reproduction of complex rhythmically-arranged groups of intervals 44

A CONTRIBUTION TO THE PSYCHOLOGY OF TIME.

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I.

HISTORICAL SKETCH OF THE PROBLEM.

It may be said that Kant and Aristotle are culminating points in the history of modern and ancient philosophy respectively. In the comparison of these two, separated by ages, we are reminded of the slow growth of knowledge ; for in essentials the philosophy of Aristotle approximates to that of Kant. Both asked the question, "How do we know ?" For answer, Kant brought the subjective and objective factors of knowledge face to face, if not together, while in Aristotle's doctrine of the categories and central sentient principle[1] there is a presentation (vague, it is true, and surrounded by crude theories) of the same factors. Prior to each were philosophers who tried to explain the universe from the standpoint of subject or object exclusively; and in succession to them there have been schools based upon the manner of interpreting their master's thought. The history of any particular problem in philosophy follows the same general lines. Arising earlier or later, according as it is included in the first cosmological inquiry or not, it takes its place in successive systems in accordance with the subjective or objective standpoint of the founder.

The time problem was not the subject of particular inquiry up to the period of Aristotle. Plato scarcely touches it, excepting in connection with his doctrine of creation, and then only in a naive manner. Aristotle asks the question, How do we perceive time? and his answer is that it is immediately perceived in connection with any and every sense perception as the number of motion.[2]

[1] Aristotle, *De Sensu et Sensili*, cap. 7, p. 449.
[2] Aristotle, *De Coelo*, c. 9, sec. 8-10.

After Aristotle down to the close of the Middle Ages, the problem was scarcely considered. In modern philosophy, from Descartes to Kant, every philosopher grappled with the question to some extent, but all were governed by their respective presuppositions, and explained time, like all else, as either wholly intellectual or wholly material. Kant's solution of the problem is, in reality, to make time one of the categories (in the Aristotelian sense of the word) or ways in which the mind looks at its content, these categories being contributed by the mind. "With regard to phenomena, we cannot think time away from them," he says, just as we cannot think "quality" and "quantity" away from them. In other words, in that unified or determined assemblage of phenomena which he calls object or fact of experience, there are two factors, the subjective and the objective, each of which is indispensable. In the subjective factor is included the temporal relation of phenomena.

Since Kant, it may be said, speaking generally, there are three schools of interpretation of the time problem; one may be characterized as the idealistic, another the materialistic, and the third is composed of those who make of time a special sensation. The first school adheres to the Kantian presentation, not merely as set forth in his chapter on Time, but as gathered from his whole philosophy. The second school is an off-shoot of one which has existed from the beginning of philosophical inquiry. Its adherents would try to show that time has an exclusively sensational basis. The third, as stated above, treats the idea of time as a special sensation. Each of these schools has its representatives in modern psychology. Dr. Mach, of the University of Prague, may be mentioned as an adherent of the last. Among those belonging to the second is Dr. Hugo Münsterberg, while as a representative of the first school the distinguished Professor Wilhelm Wundt, of Leipzig, may be taken. These three psychological theories will now be more fully stated, in the order in which the names of their representatives have been given, and a comparison will be made of their relative merits.

In the presentation of the first theory, we follow the exposition of Dr. Mach in his "Beiträge zur Analyse der Empfindungen." The contention of this school is that "time is a special state of consciousness." Mach says: "The existence of a special specific time sensation appears to me to be beyond doubt."[1] He draws his conclusion from such facts as these. In the first place, the content of time may be changed without changing the time itself. In contiguous bars of music the rhythm may be exactly the same, but the tone entirely different. The recognition of time intervals as constant, though their content be continually changing, is not a product of the reason, he says, but is rather an immediate sensation. He illustrates this position further from the striking of a clock; the separating of the strokes as first, second, third, fourth, and so on, by which their position as before and after is given, is not, he believes, a thought which accompanies the sensation of hearing, but is rather an immediate "time sensation." To explain the time sensation a little more definitely, Mach notes that it is always present in every conscious state, that it is of necessity organically connected with every conscious state. When the attention is severely strained, time appears to pass slowly; when the exertion is lighter, it passes more quickly. If we are so interested that the exertion of attention is scarcely felt, then the hours pass rapidly away. Where attention is wholly wanting, as in dreamless sleep, time passes quite unconsciously, and the only connection between the two waking periods is an intellectual tie. Besides this influence of attention on the passing of time, a very strict concentration of the attention on some expected event may actually change the time order. The blood may appear to the overstrained attention before the surgeon's lancet has actually pierced the skin. These considerations, Mach thinks, indicate such an organic connection between the time sensation and attention, and such an obedience of the former to the latter, that he concludes that "time is the work of attention." Before examining the validity of the grounds on which the above theory is based, we will state the position of those who maintain that the presentation of time is attached to *certain special* states of consciousness, characterized also by other content. Since both views are affected more or less by the same errors, they may be better discussed together.

In the presentation of the second theory, the exposition of Münsterberg in his "Beiträge zur experimentellen Psychologie" will be followed. He begins with an attack on Wundt's position, that in estimating the relative length of smaller intervals, *e.g.*

[1] Mach, *Beiträge zur Analyse der Empfindungen*, p. 104.

from 0.5 to 3 seconds, the actual time-lengths themselves are compared directly, and not through the medium of their content. Münsterberg holds that these small intervals without content do not normally occur, but that in all time presentations the judgment of the interval depends on muscular tension, that is, on tension of the different organs occasioned by muscular conduction, or by the memory of such. These are the only data at our disposal for the immediate feeling of time. Whence come these feelings of muscular tension? Goldscheider says that they have their seat principally in the joints, in the thin sensitive layer which covers the ball of the bone and the socket in which it works. Münsterberg replies that there are no joints in the eyeball, the tongue, the lips, etc., yet that the feeling of tension is quite as noticeable in these as in the leg or arm, and further, that the feeling may change after the teeth are closed and no further movement takes place in the joint of the maxillary bone. The feeling of tension arises, then, not from the local sign in the joints, but from the muscles; in the muscles lies the whole source of the feelings of tension. In stating the subjective experience on which, according to this theory, an interval is estimated, he says: "If there are given to me impressions of sight; for example, if a point of brightness should appear upon a dark ground irregularly at intervals of from one to three seconds and vanish again, I feel at every appearance of the stimulus the muscles of the eye exert themselves to direct the line of vision to the luminous spot; all the muscles contract to fix the spot firmly, and the accommodation muscle exerts itself to secure a clear impression. In short, the whole organ of vision, as soon as the stimulation begins, comes into a state of exertion, by which the stimulus gains in distinctness, and its perception rises above the other content of presentations."[1] These feelings of tension and relaxation are the whole data for the time idea, and on their nature depends the estimation of intervals. The presentation of time is the synthesis, made up of the perception of the impressions which limit the parts of time together with the sensations of muscular exertion, which increase and decrease in intensity, and which we usually do not refer directly to the muscles. We *believe*, he says, that we refer our special state of consciousness directly to time, we *believe* that we perceive time

[1] Münsterberg, *Beiträge zur experimentellen Psychologie*, Heft 2, p. 22.

sensation directly, just as the man groping in darkness believes that he perceives space directly, whereas he only perceives directly the limitations of space, and infers the rest from these directly perceived data. In the case of time, what is immediately felt is the muscular exertion increasing and diminishing in intensity, and from that time is inferred.

On the hypothesis that the estimation of intervals depends on the change in intensity of muscular sensations, Münsterberg accounts for the intelligent division of the continuous flow of time by a physical process which is continually changing, the act of respiration. In this process, the muscles of the organs involved are continuously employed in contracting and expanding, and make a regular alternation of strain and relaxation in muscular tension. Although there are factors which cause irregularities in this periodical division of time, such as the existence of certain central processes, leading us to make an inspiration at the moment we turn to an agreeable impression, and to exhale at the moment when an impression of the opposite nature is experienced, still when the attention is closely occupied inspiration and expiration follow regularly, and thus afford a continuous uniform standard of time. Before explaining the experimental work with which he supports his theory, Münsterberg notes that the discoveries of different seats of temporal sensitivity by other experimenters may not be all wrong; indeed, if the cause of their disagreement were known, it might be shown that they were all right. Their different results may be accounted for by the varying sensibility of the organs involved. In auditory impressions, an interval of 2 σ could be discriminated, but if the two stimuli were separately conducted, one to each ear, the smallest noticeable interval elapsing between the impressions was found to be 64 σ. Similarly, when an auditory and a visual stimulus were given simultaneously, the auditory impression was perceived before the visual, on account of the more rapid transmission of the stimulating process in the labyrinth of the ear. Previous theories, he says, have failed to explain this variation in the point of temporal sensitivity, but he thinks that his own, being based on muscular tension, accounts for it satisfactorily.

In his experiments, Wundt's time-sense apparatus was used, and his calculations were made by the method of average errors. He always used one normal and one comparative time interval, and in some of his experiments an interval was allowed to elapse between the normal and the comparative intervals. In the first group of his experiments, by touching the lever himself he directly stopped the apparatus, when a time which he judged equal to the normal interval had passed, and thus registered the length of the comparative interval. His trials in this first group were on intervals ranging from six to thirty seconds. First, a series of trials was made on intervals, the length of which was wholly independent of the length of time occupied by any organic function of the body; next, a series of trials was made on intervals that were multiples of his respiration time. The results of his experiments[1] show that in the first series, where the intervals were independent of respiration time, the error was 10.7 per cent., while in the second series, where the intervals occurred just in the same phases of respiration, the error was only 2.9 per cent. In a second group of trials, on intervals ranging from one to sixty seconds, the experiments were divided into two similar series. In the first series, where the phases of respiration were not taken into account, the error was 24 per cent., and in the second series, where the intervals always occurred in the same phases of respiration, the error was only 5.3 per cent. In a third group, the intervals were filled with slow regular muscular exertions and relaxations, independent of respiration. With this content in small intervals, from three to ten seconds, great errors were made, but in the longer intervals the estimation was very accurate.

These experiments prove that there is no special time sense in our consciousness. The changing muscular tension and relaxation, psycho-physically conditioned, form the standard of our time measurement. In the bodily conditioned periodicity of these exertions lies the cause for the phenomena of time. It is erroneous to attribute this periodicity to consciousness itself. "The physically unconditioned transcendental apperception does not function periodically in its apprehension of time, but those physiological excitations, whose centripetal effect is our presentation of time, are subject to periodical change. The apparently most exact proof that our consciousness is not only conscious of the psycho-physically conditioned states, but that it also interferes actively in them and changes its relation to them, has

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[1] Münsterberg, *Beiträge zur experimentellen Psychologie*, Heft 2, pp. 66 and 67.

revealed itself as a series of uncritical errors."[1] Not to the *consciousness* must the periodical undulations of the sensation of time be attributed, but to the *content* of consciousness; and no state or content of consciousness changes, unless the bodily state, *i.e.* the tension of the muscles, changes. Hence he concludes that metaphysics must again admit that a property claim of the transcendental consciousness is declared invalid and is transferred to the sensuous reflex apparatus of the body.

In the presentation of his theory, Münsterberg, like Mach, has based his conclusions on what is experimentally insufficient, and hence his theory, like that of Mach, is affected by far-reaching errors. The objection that time intervals without content do not normally occur is urged against Wundt's position that in estimating comparatively small intervals we compare the actual time-lengths themselves. This objection would be valid against Mach's contention that there is a special particular time-sensation, but it scarcely holds against Wundt, who does not maintain that there are time intervals without content, but rather that time, as a property of all states of consciousness, may by abstraction be brought into the gravitation centre of consciousness, and its relation to other properties investigated by experiment. To Münsterberg's conclusion, that the sensations arising from muscular tension and relaxation are the only data at our disposal for the immediate feeling of time, there is the fundamental objection that, since these feelings can only be experienced *in* time, they presuppose time, and must have a position as before and after other sensations. Time is one of the ways through which the mind looks at every fact of consciousness; hence to conclude that certain facts of consciousness are the data of our feeling of time is merely a *petitio principii*. Nichols has shown by careful experiments that sustained attention to a rhythm materially influences the estimation of succeeding intervals.[2] If the estimation depended wholly on muscular sensations, this would not occur. Meumann, also, has demonstrated that the intensity of the limiting sensations influences the estimation of small intervals, and that the content has little influence on the estimation of medium intervals, exercising control only in long intervals. Further, he shows that there is an æsthetic factor

[1] Münsterberg, *Beiträge, etc.*, Heft 2, p. 68.

[2] Nichols, *Psychology of Time*, p. 94.

involved, and that pleasure and pain have an effect upon the estimation of some intervals.[1]

It is far from self-evident that the above theory explains very generally the variations in the position in which different experimenters have placed the point of temporal sensitivity. The fact that an optical impression is slower in coming into consciousness than an auditory is not explained by the muscular tension, but more probably by the different nature of the processes of stimulation in the two sense-organs. The experiments on which Münsterberg bases his theory were not very accurate in method or calculation, but the amount of discrepancy arising from that source would probably not affect the results. If it be granted that his results are correct, still they do not justify the conclusions he draws from them. From the fact that intervals whose limiting sensations fall in similar phases of respiration are much more accurately estimated than others which are independent of any organic function of the body, it can only be inferred that the organic functions of the body provide data which enable the mind more accurately to estimate the intervals in which they occur. It does not justify Münsterberg's inference that the sensations arising from the tension of the muscles are the only data that we have for the feeling of time. Thus, as stated above, the experimental basis of his theory is quite inadequate. His materialistic standpoint involves, further, the fatal objection that the sensorial reflex apparatus of the body is the ultimate basis of the theory. This position is open to all the criticisms that may be urged against the empirical school. Time is nothing without the facts of consciousness, but it is a property of all facts, all processes. The error in both the theories already set forth arises from the fact that they mistake certain factors which influence time presentation for the presentations themselves.

The third position, which is the direct development of the Kantian standpoint, has as its most eminent exponent Professor Wundt of Leipzig. This theory explains time not as a special sense, nor as dependent on certain special states of consciousness, but as a factor in *all* states of consciousness. Wundt criticizes Kant for indicating, through his use of the terms external and internal sense, that there is a double experience;[2] he maintains that there is no such double ex-

[1] *Philosophische Studien*, IX., p. 264.

[2] Wundt, *Outlines of Psychology*, p. 158.

perience. From statements that appear in the *Kritik* there seems to be some ground for the above objection. No doubt Kant's training in the earlier philosophy had biassed his mind strongly in favour of the complete separation of subject and object, so that it was not easy for him to cut himself wholly free from language which might leave the impression that he himself was still enslaved to the conception of a dual experience. But Kant's system hardly justifies that interpretation. Thus, in dealing incidentally with a problem which does not belong to psychology, viz., the explanation of the community of soul and body, he says: "The difficulty which lies in the execution of this task consists, as is well known, in the presupposed heterogeneity of the object of the internal sense (the soul) and the objects of the external senses, inasmuch as the formal condition of the intuition of the one is time, and of that of the other space also. But if we consider that both kinds of objects do not differ internally, but only in so far as one *appears* externally to the other—consequently that what lies at the basis of phenomena, as acting in itself, may not be heterogeneous, this difficulty disappears."[1] This is a direct statement of the *unity of experience* —" both kinds of objects do not differ internally "—but this unity has an internal and external aspect. It is true that there is still left for Kant the " thing in itself," but the charge that he contends for a double experience even in phenomena can hardly be justified, since here, and in other passages, he evidently contends for a single experience with two factors, though it may be questioned whether he saw fully the implications of his own position.

Wundt, then, in the development of his theory, has, as the underlying principle, the " unity of experience," which seems to have been the objective point of Kant's system. Time is nothing apart from the facts of consciousness; it is not even a process, as Nichols says, but is a property of all facts and all processes. But this property can *by abstraction* be brought into the gravitation centre of consciousness, it can be brought before voluntary attention, and then its relation to other properties may be investigated by experiment. This theory is the basis of modern psychology, the science of the immediate experience; and this experience, as stated above, is not the dualism of unrelated subject

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[1] Kant, *Critique of Pure Reason*, translated by Meiklejohn, p. 252.

and object, but is composed of processes interconnected by certain laws, each of which processes has two factors, experiencing subject and objective content. The unity of the immediate experience is the self, and each fact of experience is a process connected with others, and hence implying time. Thus we see, once for all, from a psychological standpoint, that time is present in the data from which the science of psychology is constructed. Time is normally present in every fact of experience, time-relation conditions every fact of experience, but time occurs alone only as an abstraction. Time, then, is a constant factor, not only of the self, the unity of the immediate experience, but of every conscious process which goes to make up the self. By the union of sensations and feelings or affective elements, which are obtained by abstraction and analysis of the facts of experience, a psychic compound is formed, whose nature depends not so much on its component elements as upon their union. When these compounds again are united, the result is more than the sum of the compounds.

We find that time-presentations are of the nature of psychic compounds; thus it may be well to look a little more fully into the nature of psychic compounds. In the first place, they embrace all composite components of psychic experience, which have certain peculiar characteristics sufficient to make them relatively independent; next, they are processes, not things—processes which are continually changing, and can therefore only by abstraction be thought of as momentarily constant at all; and further, they are never absolutely independent, but are continuously uniting with other compounds to form a greater interconnection. Compounds are of two classes according as they are formed principally of sensational or of affective elements. Time compounds belong generally speaking to the former class, though not exclusively, for certain affective elements play an important part in their formation. There are two most favourable sources for temporal ideas, the so-called inner-touch sensation, and auditory sensations, though the conditions for their use are present in every sensation. The inner-touch sensations, especially those accompanying the movements of the body which are most involuntary, such as walking, form a basis for the ideas of time. In every step taken there is a series of sensations, repeated for every subsequent step in exactly the same order. The beginning and the end of each step are characterized by outer tactual sensations, caused by lifting the foot and replacing it again, and between these two limiting points there is a regular series of inner-touch sensations, arising from the movements of the hip joints, and these are stronger at the beginning and the end on account of effort and inhibition. Corresponding to the inner-touch sensations we have a series of affective elements consisting of feelings of strained expectation followed by feelings of fulfilment. Here, then, are two elements of the time idea, sensational and affective, the former predominating. In examining auditory sensations we find that the sensations which go to make up the idea are only at the end of single intervals. But although there is an absence of almost all objective sensation in these intervals, there is a very noticeable affective content, viz., the gradually increasing feeling of strained expectation followed by the feeling of fulfilled expectation. There is also a less noticeable, but still important, sensational element in the content, arising from the tension of the tympanic membrane and other parts of the organ involved; the subjective element is, however, by far the most important in the auditory sphere, and its influence on the time idea has been shown to be quite fundamental. It has been found that an interval of one-fifth of a second is most favourable for the union of successive auditory impressions,[1] a fact which shows that the continuous recurrence of this interval secures the most emphatic alternation of the subjective elements of strained expectation and fulfilment. If the interval be longer, the feeling of strained expectation is so intense as to be unpleasant, and if it be shorter, the rapid alternation of feeling becomes fatiguing. We have in this a proof of the influence of an æsthetic element in the judgment of the time interval. Analysis thus proves that the elements of the time idea are both sensational and affective. They are not mutually exclusive, nor can temporal attributes be ascribed to either of them separately. We therefore conclude that the time idea is a fusion of the two elements with each other and with the objective impression. In conscious life we always find them so fused, so that again after analysis we arrive at the position from which we started, that time is a constant factor of conscious life, and that there is no such thing as isolation in the continuum of consciousness.

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[1] Wundt, *Outlines of Psychology*, p. 149.

In the time idea the elements bear a fixed unchangeable relation to one another; none can be changed without changing the whole. But in addition to this the elements bear a relation to the ideating subject, so that when any element is changed in relation to another it also changes its relation to the ideating subject. These two relations are always found connected in actual experience, but when we isolate them for the purpose of investigation, we find that from the relation of the elements to one another we get the so-called modes of time, viz., brief, short, long, etc., and from their relation to the ideating subject we get the temporal stages, past, present and future. If a series *a*, *b*, *c*, *d*, etc., represent points in space, they may all be perceived at once; the fixation point may rest on any one of them, and the rest be grouped from that centre. If it be a time series, they may also be perceived at once, if they be points in the past, but the fixation point of time is always the present moment, and from this point the other points in time are always arranged. Since the present moment is always judged in relation to the totality of the past, then it follows that no two moments are identical, for the totality of the past with which one moment is related is increased by that moment and thereby altered before the next moment is related to it. Thus there is a characteristic belonging to every point in time, which distinguishes it as past in relation to the present moment, and makes this present the fixation point from which all the rest are arranged. The fact that the affective element in time is conditioned by the totality of previous experience, and so made different, produces an ever-changing impression of the present moment. This impression of the present moment is called the inner fixation point, and the never-ceasing change in the inner fixation point is called the continuous flow of time. This continuous change means that no moment of time is like any other moment, and that no moment can ever return. There is no such thing as time in which no change takes place, and since the fixation point is continually moving forward it follows that time is of a one-dimensional character.

The above is in substance Wundt's theory of time; it is the theory of modern experimental psychology. And let it be observed that although by abstraction and analysis the time idea has been reduced to its elements for the purpose of investigating its nature, still it is only by artificial analysis that it can be so separated into its elements. In nature the time idea does not appear so separated, but only as a property of all facts of consciousness. This theory avoids the errors which Mach falls into in making the time idea a special particular sensation, and also the errors of which Münsterberg is guilty in making the time idea the property of a special sense only, and in working out his theory ultimately from a materialistic standpoint. Wundt's theory, starting as it does from what we hold to be the only consistent standpoint, viz., immediate experience, will, we believe, account for all the phenomena of time.

II.

THEORY OF TIME ESTIMATION.

In all states, the percipient subject is conscious of a temporal relation; no experience is without duration. Both sensations and the intervals between them occupy time; each has a content, but the content of a sensation is more evident than that of an interval. The duration of sensations has not been experimentally investigated in regard to its temporal value for consciousness. Experiments on the estimation of time in intervals, by Külpe, Meumann, and others, show that below an interval of a certain length the limiting stimuli exercise an important influence. Above this interval, where longer periods are involved, the content of the interval, *e.g.* respiration, heart-beat, or some other organic function, enters as a factor influencing the judgment.

The interval that lies between the short and long groups, which has been found to be the most accurately estimated interval, we believe to be the "unit of time," and the basis of all time estimation. It differs in length more or less in different individuals, but in all cases the constant error and mean variation are less as we approximate to it. Here, neither the limiting impression, which influences the judgment of shorter intervals, nor the organically functioned content, which influences the judgment of longer intervals, modifies the estimation to any marked degree. This remarkable fact in connection with the estimation of intervals, that a more or less definite interval is estimated most accurately, and the further fact that as we depart from it in either direction errors arise in judgment, lead us to believe it to be the basis of the estimation of intervals in all cases—a temporal unit. This theory of a unitary basis of estimation, moreover, is in accordance with that demand for single principles, which is fundamental in the human mind. With dual principles the mind is never satisfied, and it was for this reason, in part, that the theory of a three-fold basis of estimation advanced by Külpe[1] was rejected. The theory of a temporal unit is therefore in harmony with the scientific demand for explanation by as few principles as are consistent with facts. That this theory has an experimental basis, we shall now proceed to show in the three cases of the unitary interval, of intervals below, and of intervals above the temporal unit.

1. In the estimation of the unitary interval, the interval itself is the basis of estimation, as Külpe, Meumann, and others have said. Here, also, its more or less definite content (obtained by analysis) of feelings of expectation and relief, and of muscular sensation, is scarcely noticed. Some investigators would make this content the basis of time estimation. Among these, Schumann, whose theory[2] will be examined later, may be mentioned. The estimation of this interval, too, as would be naturally expected, is more accurate than that of any other.

2. In the estimation of intervals below the temporal unit, the experiments of Külpe and Meumann revealed a tendency to reproduce these intervals longer. Such was also the case in our own experiments.[3] This error in estimation seems to be due to the tendency of the temporal unit to complete itself, it being the individual basis of estimation. It will then be made up of both the empty interval and the limiting stimuli; and the judgment will be affected more or less by the nature of these, but the over-estimation becomes less and less as the interval approaches the length of the unit of time. This tendency to complete itself, on the part of the unit, is confirmed by results of other investigations. In experiments on the time relations of poetical metre, a tendency to complete the foot, where the metre was irregular, was noticed.[4] In this connection, we may note that Leuba[5]

[1] Külpe, *Outlines of Psychology*, sec. 65, 3.

[2] *Zeitschrift für Psychologie*, Bd. xviii., p. 1.

[3] Cf. Tables III., IV. and V., *infra*.

[4] See "*Experiments on the Time Relations of Poetical Metres*," by Messrs. Hurst and McKay, in this volume.

[5] *Psychological Review*, 1898, p. 483.

suggests, as an explanation of the quantitative change in memory, that there is a typical representative for each class of experiences (conscious processes), which is a residuum left over from all the experiences we have had. Towards this middle type or unit all our memory images are drawn.

As was said above, the nature of the limiting stimuli is a factor which enters into the estimation of intervals smaller than the unit, and has a bearing on the accuracy of their reproduction. It is found, for example, that intervals which are bounded by a fairly loud, sharp sensation are judged to be shorter than intervals bounded by a weak sensation,[1] while on the other hand they are judged to be shorter likewise than intervals whose end-signals are still stronger.[2] This difference is accounted for by the subjective strain of attention or expectation in the weak and also in the very strong sensations, which tends to lengthen the intervals. From these facts, Külpe[3] concludes that the limiting stimuli are the basis for the estimation of short intervals, whereas they are but a disturbing factor in the estimation. Schumann, in the same way, makes the feelings of strained expectation and surprise negotiate the estimation of the interval. Intervals shorter than the unit of time, however, cannot be estimated and reproduced individually, but only by being grouped into a series. This is confirmed by the fact that they are thrown into a subjective rhythm, which divides itself according to the temporal unit or multiples of it. Rhythmic reproduction of short intervals is established by the results of Meumann, Külpe, Müller and others. We do not believe that such rhythmic grouping is confined to intervals limited by auditory impressions alone, as Külpe says.[4] Experiments were made by students under the direction of Professor Kirschmann, in the laboratory of the University of Toronto during the session 1894-95, with the following recorded results.[5] Mr. Warren, under the heading *Pendulum Experiments*, writes: "In the first experiment, a striking rhythmic interval occurs, showing itself from the first and continuing with the same regularity during the reproduced time, the periodic return of under-estimation and over-estimation increasing during the

[1]Cf. Tables IV., V. and VI., *infra*.

[2]Wundt, *Outlines of Psychology*, p. 151.

[3]Külpe, *Outlines of Psychology*, sec. 65, 3 (i).

[4]Külpe, *Outlines of Psychology*, sec. 65, 5, 2a.

[5]These results are unpublished, but they are preserved in the Archives of the Psychological Department.

progress of the experiments." Mr. Crawford's experiments show a noticeable, although not a constant, periodicity in visual impressions. Under the heading *Points to be Noticed*, he says: "In experiments with visual impressions (pendulum vibration), a periodicity is noticeable in some series of experiments, but not in all." Mr. F. W. Varley, in a very carefully prepared paper, summing up results of experiments with both visual and auditory stimuli, says: "On viewing the graphic representations of these experiments, it may be seen that they have been characterized by waves of attention. Periodically there has been a comparatively great aberration, indicating a relaxation." In our own experiments with visual impressions, the intervals ranged from half a second to nine seconds, and the tendency to rhythmic accentuation was not noticeable. In nine series of experiments with auditory (metronome) impressions, there was a marked periodicity in some, while in others it was not apparent.

From the above statements we do not think that the exclusion from rhythmic accentuation of all intervals except those bounded by auditory impressions is warranted. On the contrary, it seems to be a constant characteristic in the reproduction of all short intervals, and is due, in part at least, to the fact that such short intervals cannot be estimated individually, but are thrown into a subjective grouping. Thus the experimental facts in connection with the estimation of the class of short intervals supply a basis for the theory of a temporal unit.

3. In the estimation of intervals above the unit of time, observers find great, almost invincible difficulty in excluding the influence of the content. This content is generally some regularly recurring organic function, and upon it depends largely the accuracy of the judgment. The observer K. attempted to exclude the influence of content as far as possible. The results show that this attempt vitiated his judgment to a very marked extent. The explanation is not, however, that the content is the basis of judgment, but that the mind estimates intervals longer than the unit in terms of the unit itself, and that the unit or the multiple of it is here represented by some regularly recurring organic function. As in short intervals reproduced in groups the quality of the limiting sensation is a factor entering into the estimation of the interval, so in long intervals the character of the content plays a part in the estimation. An interval which has a full and varied content is judged to be comparatively

longer than one which is not so crowded, because of the strain required to keep the attention on the interval, when there are other things to distract it. If, however, the content becomes so interesting as to absorb the attention entirely, the time passes more quickly and is judged to be shorter. The part thus played by attention does not make it a basis of estimation, as Mach thinks, but only shows its influence as a single factor.

We may now sum up our conclusions as follows:

(1) A certain interval, which varies considerably in different individuals and which may vary in the same individual at different times, is the unit of time and the psychological basis of the estimation of intervals in all three classes.

(2) In the first and third classes of intervals factors enter which influence the judgment, but they are only modifying conditions, not the bases of our estimation. There are, in our opinion, not three different kinds of temporal judgment, but three applications of the one form of temporal judgment.

(3) Although time must be considered as an elementary property of consciousness, incapable of further analysis, the unit of time, nevertheless, which seems to be the basis of the estimation of intervals, is of the nature of a psychic compound. It contains, as may be seen by analysis, both subjective and objective factors. Of these two factors, the objective seems to have, at least genetically, the predominating influence. It may be some organic function, or multiple of such, which has accustomed us to the duration which is our unit more than to any other, or the duration of some objective impression or interval to which we have been always accustomed may have played the fundamental part in its formation.

Before proceeding to give any account of our experimental work in the laboratory of the University of Toronto, it may be well to consider briefly two recent articles on this subject of time judgment by F. Schumann.[1] The views there presented might be classed with the second of the time theories given above (p. 7) and passed without further comment, for they partake of the same general nature, being one-sided and materialistic in tendency. The views are, however, the most recent exposition of the school, and for that reason we consider them a little more fully.

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[1] *Zeitschrift für Psychologie*, Bd. xvii., p. 106, and Bd. xviii, p. 1.

Schumann maintains that in the estimation of time intervals the judgment is mediate.[1] Whether it is mediate or immediate must be answered, he says, by experimental psychology, and not from theoretical principles, and he concludes from his experiments that the feelings of expectation and surprise mediate or negotiate the estimation of the interval. Without following the steps by which he reaches this conclusion, to be noted later, we shall first examine the basis from which he starts and his theory of judgment. The starting point for psychology, according to Schumann, must be "inner perception."[2] The latter term is used for the perception which arises on the occasion of a sense stimulation. In making this the basis of certain knowledge Schumann is unduly emphasizing the subjective, almost to the exclusion of the objective element of the immediately given fact of experience. The immediately given is the idea-object, as Wundt has shown, and not the idea, which must be the element of inner perception. The idea and the object are secondary products, the result of the analysis of the immediately given fact. Hence Schumann's "inner perception," which he assumes to be the immediately given, and therefore the basis of certainty, is not the immediately given, but an abstraction. However valuable the accuracy of his experimental investigation may be, the value of the conclusions he arrives at will be, if not vitiated, at least much impaired, on account of this erroneous basis of certain knowledge from which he starts. Schumann's state of confusion as to the nature of the immediately given is again apparent in his theory of the judgment of time intervals. In his conclusion[3] that expectation and surprise mediate or negotiate the judgment of time intervals, he is using two complex temporal processes as the basis of his judgment. Hence, on his own showing, time estimation is not a judgment mediated by untemporal processes, differing entirely in nature from time itself, but it is rather of the nature of an immediate comparative judgment. For those processes, which he says mediate the judgment, in so far as they play a part in that judgment, do it through their own time element, which in the act of judgment is compared with the respective time intervals under discussion.

Schumann seems to leave out of consideration, in his theory of

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[1] *Zeitschrift für Psychologie*, Bd. xviii.

[2] *Zeitschrift für Psychologie*, Bd. xvii.

[3] *Zeitschrift für Psychologie*, Bd. xviii.

judgment, the relativity of the whole content of consciousness. He maintains that there is no comparative activity in judgment, on such grounds as that the intensity of an impression advances a number of units before the judgment of increased intensity arises. But, if there be no comparison, to say that the intensity is increased means nothing. It must be increased in relation to some other intensity. Further, if there be no comparison in judgment, then Schumann's theory that expectation and surprise mediate the judgment cannot be sustained, for in this process of mediation there is comparison involved. Either Schumann must give up his theory of judgment being entirely passive, or else he cannot maintain his theory of time estimation. A clear comprehension of experimental psychology, the standard to which Schumann appeals, should show that the most fundamental and primary judgment involves activity from one point of view, and passivity from another. For the more or less relatively isolated immediate experience may be viewed from one point as the active knowing, and from another as the passive known. These are not two different experiences, but two ways of looking at the one unitary experience. The most fundamental judgment, then, is both active and passive, and every other judgment must partake more or less of the same nature.

Having shown what we believe to be the errors of Schumann's theory of judgment, and that too by an appeal to the same standard which he himself applies, viz., the decision by experimental research, we shall now consider his theory of the estimation of small intervals. Schumann says: [1]"Ich habe nun die Ansicht ausgesprochen und zu beweisen gesucht, dass diese Nebeneindrücke der Erwartungsspannung und der Ueberraschung die Schätzung der Intervalle vermitteln und zwar in der Weise dass ein Intervall, vor dessen Endsignal eine lebhaftere Erwartungsspannung auftritt, länger erscheint als ein Intervall, bei welchem sich nur eine schwächere Erwartungsspannung geltend macht, und dass jedes durch Erwartungsspannung ausgefüllte Intervall für länger gehalten wird als ein Intervall dessen Endsignal unerwartet kommt." We have stated in a previous part of this paper what place these feelings of strained expectation and surprise occupy in the estimation of intervals. That they are not the basis of our estimation seems evident from the following considerations:

[1] *Zeitschrift für Psychologie.* Bd. xviii., p. 2.

1. There is one fact in connection with the estimation of intervals which has been experimentally established almost beyond a doubt, viz., that there is a certain more or less definite interval which is most accurately estimated. It is admitted by investigators that in the estimation of this interval these accompanying impressions of expectation and surprise are less marked than at any other. Why is it then, if they negotiate the estimation of the interval, that where the sensible discrimination is most accurate we are least conscious of these feelings? If they negotiated the estimation of intervals they should be most marked where that estimation is most accurate. That this is not the case has been experimentally established. Not only are they not marked where the estimation is most accurate, but according to Schumann[1] himself, when the strain of attention is strongest, and surprise greatest, there the greatest errors in judgment are made. This error, he goes on to say, becomes less as the feelings of expectation and surprise decrease. Hence we must look deeper than these for the basis of the estimation.

2. It is as clearly established as the fact of a most accurately estimated interval, that above and below this interval there is a gradually increasing error in judgment. This fact of the over-estimation of intervals shorter than the one we have called the unit of time, and the under-estimation of intervals longer than the unit, is not explained by Schumann's theory. If these negotiated the estimation of intervals, it would follow that wherever they are as little noticed as at the unit of time the estimation should be just as accurate. But that this accuracy does not occur has been established by experiment; for, both above and below the unit of time, after the strain consequent upon the change of normal time has passed away, when the attention has adjusted itself and the end-signal enters just when it is expected, there is ever found an error in judgment which increases as we depart in either direction from the most accurately estimated interval. Hence we conclude that the feelings of expectation and surprise do not mediate the estimation of the intervals, either when these feelings are most marked, or when they are at the mean of alternation. We do not deny their presence, but they are only factors in the estimation, not its basis.

3. In addition to the above reasons for rejecting the theory that makes time estimation depend on feelings of strained expec-

[1] *Zeitschrift für Psychologie.* Bd. xviii., p. 2.

tation and surprise, there is another, and not one of the least important. It has been mentioned above in connection with the criticism of Schumann's theory of judgment. Wherever a feeling of strained expectation is experienced a complicated process takes place, which has temporal relations and can itself be estimated. This feeling of strained expectation is one of the affective accompaniments of any content of consciousness which is being actively apperceived. It is exceedingly complicated and may be accompanied with excitement, pleasure, pain and the like, and its temporal relations may be of longer or shorter duration. Yet we are asked to take as the negotiator of temporal estimation a complicated affective process which is itself as temporal as that which we are estimating!

III.

EXPERIMENTS ON THE EFFECT OF A LAPSE OF TIME BETWEEN THE NORMAL AND COMPARATIVE INTERVALS.

The purpose of the experiments now to be described was to investigate the effect of a lapse of time between the normal and the recording of the comparative interval upon the accuracy of the reproduction. The work on these experiments was done at regular times of the day and week, so as to secure as far as possible uniform conditions in the subject, and the experiments were continued over a period of four months. Everything was removed that might create any counter-interest, or in any way disturb the subject whilst making the judgments, so that, as far as objective conditions were concerned, there was nothing to hinder the subject from giving his undivided attention to the work.

The apparatus used was a Charles Verdin kymograph (Paris, 1892), with a number of fixtures designed by Professor Kirschmann, for the purpose of securing visual and auditory stimulation, and to insure accurate recording of the judgments of the subject. Visual stimulation was used exclusively in these experiments, and the method by which the light-flash was communicated to the observer may be seen by referring to the half-tone cut of the kymograph (Fig. 1.) The flame of an incandescent lamp was reflected from a small mirror (C in Fig. 2) at the end of the drum of the kymograph, through holes cut at equal distances in a band-shaped ring which was fitted on to the end of the drum. As the drum revolved the reflected light was seen through a telescope by the observer, who sat about fifteen feet away. The light appeared in successive flashes as the holes passed the mirror. The length of the interval was varied by changing the number of open holes. The field of vision of the observer for the flash of light was entirely obstructed except for what appeared through a graduated aperture in the screen A (see Fig. 1.), in front of the revolving band. The lamp B was enclosed, so as to throw not much light out into the room, which was darkened during the experiments; the light flashes would therefore be sharp. The comparative intervals were registered on the carboned paper which covered the drum of the kymograph, by means of a rigid steel wire, one end of which the observer held between his fingers, and the other end, passing round a pulley, was connected with the pointer C in contact with the paper. At the pull of the observer the pointer slid along steel rods, thus making a curve in the otherwise regular line, and by this means the estimation of the subject was recorded. A weight (D, Fig. 1) connected with the pointer brought it back to its place as soon as the strain on the wire was released, after each reaction.

The use of simple mechanical transmission might at first glance seem primitive, but, in accord with the request of the director of the laboratories, we wished to avoid all complications arising from the use of electricity. The current and silently accepted view that electricity, since it travels so swiftly, requires also but an exceedingly short time to accomplish work, *i.e.*, to overcome resistance, is utterly fallacious. Wherever electricity has to accomplish a mechanical labour, be it in the form of chemical or electro-magnetical action, or simply the discharge of a spark, it requires time; and this time is not independent of the duration of previous conditions.[1] In the case where longer periods have to be recorded, and where efficient controlling apparatus can be applied, as in Wundt's chronograph, the errors mentioned do not amount to much; but wherever short and varying intervals have to be recorded, the errors produced by electric marking will materially impair if not wholly invalidate the results.

[1]Külpe and Kirschmann, *Ein neuer Apparat zur Controlle zeitmessender Instrumente.* (*Phil. Stud.*, Bd. viii., p. 145.)

In order to control the speed of the revolution of the drum, and to serve as a basis of measurement of the normal and the recorded intervals, the vibrations of a König tuning-fork were excited by an electric current from a storage battery, and transferred to the carboned paper on the drum by a Deprez signal (D, Fig. 2). The steel point of the Deprez signal, which was used to record the vibrations of the tuning-fork, was mounted on the same rods, but just behind the pointer which the observer used to record his judgments. The lines made by the two points thus follow each other side by side round the drum, except where the straight track of the pointer is interrupted by the curve marking the reaction. The Deprez signal and the pointer passed gradually from one end of the revolving drum to the other, by means of a screw connection (E, Fig. 1) worked by the assistant.

The experiments of this investigation, the results of which are given in the tables below, were made by S. on intervals of half a second, three-quarters of a second, and a second and a half. In order to avoid the errors of reaction time involved in experiments where a single normal and comparative interval are used, a successive series of intervals was used in both cases. The observer was allowed before each series of judgments to see the objective stimulation for half a minute, and in order to assist in getting the rhythm of the successive stimuli, where necessary, some movement of the hand or foot was used. When the half minute had elapsed the objective stimulus was turned out, and the observer began immediately to record the series of comparative intervals, continuing until stopped by the assistant. The same series was exactly repeated twice again for each interval tested, with only this change, that half a minute and one minute respectively were allowed to elapse between the cessation of the objective stimuli and the beginning of the recording of the compared intervals. During this lapse of time the physical movement referred to above was discontinued. With the intervals of half a second and one and a half seconds three groups of the series were made, but only two with the three quarters of a second interval. In the first table below is given a summary of the experiments. Only the first twelve trials in each case are counted, making in all thirty-six trials at the "immediate," half minute, and one minute pauses with the half-second and second and a half intervals respectively, and twenty-four trials at the same three pauses with the three-quarters of a second interval. The second table is a summary of all the trials made. The normal, average, constant error, and mean variation are given in terms of one-hundredth of a second. The first table is given with an equal number of trials in each group of the series, lest the experiment should be vitiated by having an unequal number of trials at the various lengths of pause.

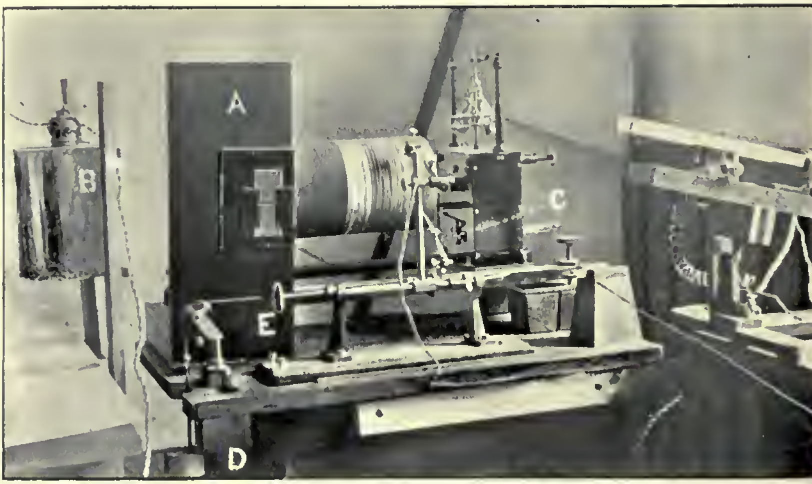

FIG. 1.

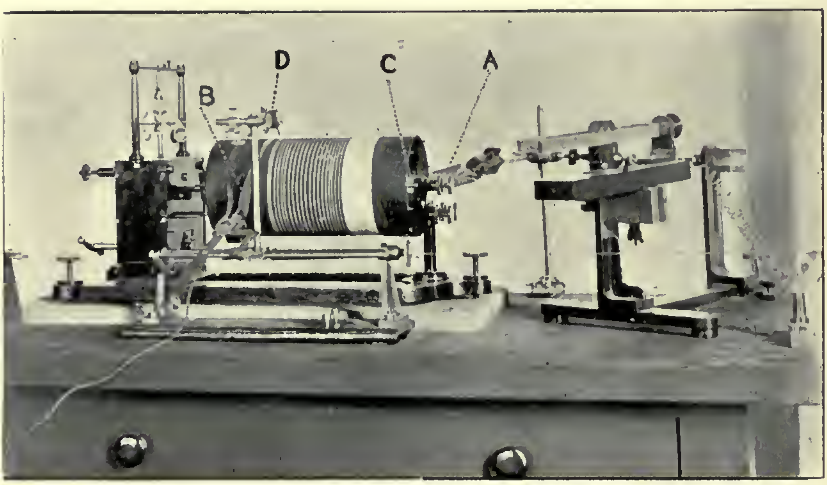

FIG. 2.

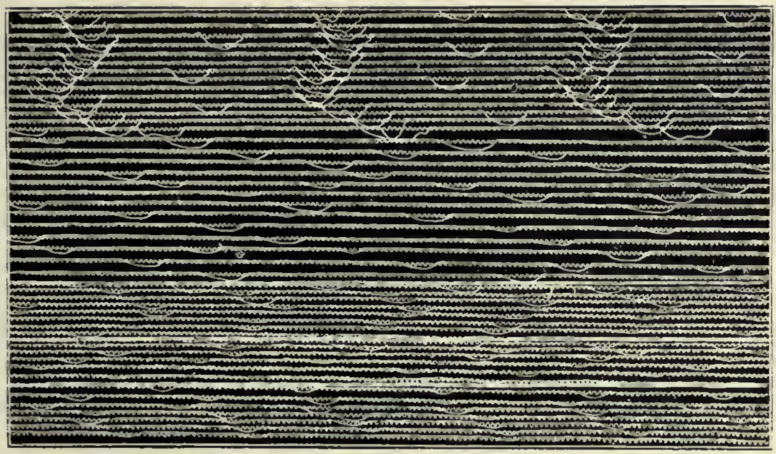

FIG. 3.

TABLE I.

Normal.	No. of Trials.	Length of Pause.	Average.	Const. Error.	Mean Variation.
50	36	Immediate	47.27	— 3.73	1.90
50	36	½ min.	45.98	— 5.02	2.57
50	36	1 min.	47.69	— 3.31	1.90
75.5	24	Immediate	66.91	— 8.09	2.60
75.5	24	½ min.	65.12	— 9.88	2.91
75.5	24	1 min.	66.91	— 8.09	3.52
153.66	36	Immediate	132.60	—21.06	4.02
153.66	36	½ min.	136.26	—17.40	6.12
153.66	36	1 min.	132.60	—21.06	10.25

TABLE II.

Normal.	No. of Trials.	Length of Pause.	Average.	Const. Error.	Mean Variation.
50	64	Immediate	45.96	— 5.04	1.96
50	87	½ min.	45.31	— 5.69	2.40
50	87	1 min.	47.42	— 3.58	2.25
75.5	54	Immediate	67.33	—10.17	3.05
75.5	60	½ min.	65.7	—11.81	2.40
75.5	41	1 min.	96.59	—10.91	3.49
153.66	38	Immediate	131.96	—21.60	4.04
153.66	47	½ min.	134.79	—18.87	6.38
153.66	54	1 min.	129.47	—24.19	9.63

Results. (Taken from Table I.)

(1) The interval of half a second approximates to what has been called above, the unit of time. The constant error and mean variation are less at this interval than at any of the others. Here also the effect of pause is less than with intervals above it. The mean variation is the same for "immediate" and "1 min.", while the constant error is nearly the same for both.

(2) With the intervals above the unit of time the mean variation increases with the length of pause. Sensibility decreases with intervals above the unit.

(3) The constant error is practically the same throughout for "1 min." and "immediate."

(4) After the lapse of half a minute the results are not regular; with the longest normal interval the constant error is less than for "immediate," but with the other normal intervals it is greater. The mean variation is always greater for "half min." than for "immediate."

The results from Table II. do not vary much from the above.

IV.

WEBER'S LAW AND TIME ESTIMATION.

The limit of the sphere in which Weber's law holds accurately is still an open question. Sanford[1] says that it holds "tolerably" for medium stimuli of several kinds, with very large and small stimuli only imperfectly, and with some it cannot be demonstrated. Külpe[2] says that with pressure, auditory, and strain sensation, also with light intensities, a constancy of sensible discrimination is experienced. The scanty investigation of the senses of temperature, taste, and smell has not yet reached a stage at which definite conclusions can be drawn. Wundt[3] says that the law has been shown to hold for the intensity of sensations, and within certain limits for the comparison of extensive compounds, especially temporal ideas; also to some extent for spacial ideas of sight and for motor ideas. On the other hand it

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[1] Sanford, *Experimental Psychology*, Ch. viii., p. 334.

[2] Külpe, *Outlines of Psychology*, p. 126.

[3] Wundt, *Outlines of Psychology*, p. 255.

does not hold for spacial ideas of external touch, obviously on account of the complexity of the local signs, and it cannot be verified for sensational qualities.

The above authorities do not differ much among themselves, but there is a great disparity in the direct testimony of those who have investigated the validity of the law in reference to time estimation; and on this account our own series of experiments was undertaken. The great difference in the testimony of earlier investigators has probably arisen from their having failed to take into account correctly the many disturbing factors which may vitiate the results of experiments.

One such factor is the general condition of the operator; fatigue or nervousness may be the source of much divergence in the testimony of subjects at different times. The state of attention also is a source of considerable variation; a concentrated, well-governed attention will give more accurate judgment than a musing, listless or uncontrolled attention. This factor has been treated in detail above. The method of registering experiments has led to erroneous results, as noted in connection with the work of Mach and Münsterberg. A lack of appreciation of the true function of rhythm in time estimation has led many experimenters to regard it as a factor which detracts from the accuracy of the estimation of intervals, instead of as a condition which makes the estimation of certain very short intervals possible. The effect of aesthetic factors too has been often misinterpreted, as has also the part played by the content in the estimation of larger intervals. These factors, which are all involved in the estimation of time intervals, are mentioned to show the many sources from which confusion may arise in the treatment of the question. But if these, and perhaps others not noted, could be carefully eliminated from the experiments, there would probable be no such disparity in the testimony of investigators as to the validity of Weber's law in the estimation of time intervals.

Ernst Mach, in a series of experiments which are neither fully nor definitely enough stated to sanction his conclusion, finds that Weber's law is not valid. His experiments were made from 1860 to 1865, extending over a period of five years, during which the conditions were not very uniform, his rather primitive methods of stimulation and recording being changed from time to time. He professes to have tested intervals from .016 sec. upward, but he only gives figures for a few of the intervals. Taking such a small interval as the lower limit, and testing the validity of the law from the whole range of intervals, from one so very short to a long one, he could scarcely have come to any other conclusion, since such small intervals, even if they could be individually estimated, could not be recorded with any degree of accuracy. Hence little importance can be attached to the results at which he arrived.[1]

Karl Vierordt maintains that the law does not hold. He thinks that the fact of contrast between long and short intervals, making the former longer and the latter shorter, is sufficient to cause a discrepancy, and that this, together with some other factors, renders uniformity in time estimation impossible. Though Vierordt is right in maintaining that after a succession of short intervals a long one seems unduly long, still that fact is not sufficient to destroy the correctness and uniformity of time estimation. His experiments, moreover, were too limited in number to be very conclusive.[2] Volkmar Estel concludes with Mach and Vierordt that the law does not hold. He experimented on intervals ranging from 1.5 to 8 seconds, and his recording was done with the earlier form of Wundt's time-sense apparatus. His experiments were too scanty to be of real value; some of the intervals received only three trials by each subject, and few were treated more thoroughly. In addition to this, his work is coloured by such a strong controversial spirit, arising from his dispute with Vierordt as to the effect of a pause between the normal and comparative intervals, that his results cannot be greatly relied on.[3] Fechner subjects Estel's work to a severe criticism, and exposes its weakness. He himself thinks that nothing is to be discovered which is irreconcilable with Weber's law.[4] Mehner comes to a conclusion midway between Estel and Fechner; he finds that the law holds approximately above the interval of 7.1 seconds, where the sensibility is constant, but that below 7.1 seconds, where the sensibility is rhythmic, it does not hold. He maintains that in the results of Estel and others the disparity of results arises from lack of practice, strain of atten-

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[1] *Untersuchen über den Zeitsinn des Ohres.* (Moleschott's *Untersuchungen*, 1866, p. 181.)

[2] *Der Zeitsinn.* (Tübingen, 1868).

[3] *Neue Versuche über den Zeitsinn.* (*Phil. Studien*, Vol. ii., p. 37.)

[4] *Über die Frage des Weber'schen Gesetzes und Periodicitätsgesetzes im Gebiet des Zeitsinns.* (*Abhandl. d. mathemat.-phys. Classe d. kgl. Sächs. Gesell. d. Wiss.*, Vol. viii. p. 3.)

tion, etc.[1] Richard Glass found that there was a tendency to follow the law,[2] and the results of Ejner also show that the increase of the average error with the length of the interval approximately follows Weber's law. A weakness in his experiments is that they were practically all conducted on one subject, but otherwise they were sufficiently comprehensive.[3] Thorkelson maintains that Weber's law is entirely valid throughout in the judgment of time intervals.

We proceed now to our own experiments. We first conducted a series, extending over four months, for the purpose of investigating the influence exerted on the reproduction of an interval by a lapse of time between the normal time magnitude and the recording of the comparative interval, the results of which are stated above.[4] The work on that problem practised us and gave us a thorough acquaintance with the apparatus used for recording the estimated length of intervals, and with the method of conducting experiments similar to those required for the testing of Weber's law. On this account a smaller number of trials was made in the latter investigation than would have been necessary to draw any valid conclusions, had the ground been entirely new. Our first aim in the experiments was to remove as far as possible all factors which might have a disturbing influence on the accuracy of the judgment of the subject; since these seem to have been responsible in many cases, probably to some extent in all the work of previous experimenters, for the disparity of their testimony in regard to the validity of Weber's law. To this end, in order to secure the same physical state, and avoid the influence of fatigue with its concomitants of musing, listless or laboured attention, the experiments in 1897-98 were made regularly between the hours of eleven and one. In the following academic year 1898-99 the experiments were made regularly between the hours of two and four in the afternoon. All disturbing factors which might interrupt the subject during the course of his work were removed, and there were, as far as could be ascertained, no special or unusual aesthetic influences present to bias his judgment.

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[1] *Zur Lehre vom Zeitsinn.* (*Phil. Studien*, Vol. ii., p. 546.)

[2] *Kritisches und Experimentelles über den Zeitsinn.* (*Phil. Studien*, Vol. v., p. 423.)

[3] *Experimentelle Untersuchungen über den Zeitsinn.* (Inaugural Dissertation No. 137, Dorpat, 1889).

[4] *Supra*, p. 30.

For the last division of the three following tables, the method of visual stimulation and the apparatus for recording the estimation described above were used. But in the other divisions of the tables, where the intervals were too short to admit of the same system, an auditory stimulation was arranged, and also a modified method of recording. The stimulation was made by means of little pegs fitted into sockets corresponding to the holes in the band on the end of the drum through which the light flashes were reflected. As the drum revolved, these pegs, in passing, struck a small aluminum plate (A, Fig. 2), and so produced a series of regular clicks. The length of the interval between the clicks was varied by changing the number of pegs. The method of registering these intervals may be seen by referring to Fig. 2. Instead of the pointer described above for writing the records, another was substituted, having the same connection, and the same relation to the Deprez signal, but extending below the rods on which it travelled. This pointer (B, Fig. 2) moved on a pivot through its centre, so that the observer, by striking the lower end with his finger, made a slight movement of the other end of the pointer, which was in contact with the carboned paper, thus causing a slight divergence in the line and so recording his judgment. The pointer was immediately drawn back to its place by a spring.

In experiments which have been made with single, normal and comparative intervals, the reaction time of the subject materially reduces the accuracy of the judgment registered. This source of error was avoided in our experiments by making both the normal and the comparative consist of a series of immediately succeeding intervals. Four persons were tested on intervals ranging from 0.1 to 9 seconds. A large number of trials were made on each of the intervals, by each subject; in all six thousand nine hundred and thirty-five judgments were recorded. The exact distribution of the judgments and intervals may be seen from the tables below, in which the experiments are summarized. Throughout the experiments, on both short and long intervals, in order to get the interval definitely into consciousness, before recording the judgments, twenty practice reactions were made immediately before each series, followed by from twenty to forty reactions recorded on the drum of the kymograph. This plan was followed throughout the experiments, and it acted as a safeguard against the errors which so frequently arise from diffused attention, and from the nervous confusion frequently involved in the first few reactions. The accompanying cut (Fig. 3) represents parts of three sample sheets taken from the drum of the kymograph, containing the tracings of a tuning-fork of one hundred vibrations to the second, by which the length of the interval was registered, and the tracings of the pointer which marked the limits of the estimated interval.

We give next a sample sheet containing the readings in detail of the length of each estimation of the interval, together with their average and mean variation, the latter representing the average error. In the tables that follow appear only the summed results of the different series of estimations made by each subject in the experiments.

Reading of Sheet 17, Observer K.

Visual stimulation. Two holes open in the drum of the kymograph, through which the light stimulation flashed to the observer at each revolution of the drum.

Three series of trials, a short stimulation each time, then twenty practice reactions with objective stimulation, followed by twenty reactions without objective stimulus, recorded on the carboned paper on the drum.

Normal interval 0.75 seconds.

Series I.			*Series II.*			*Series III.*		
68	−	$8\frac{7}{20}$	73	−	$2\frac{1}{18}$	83	+	$8\frac{1}{5}$
87	+	$10\frac{13}{20}$	73	−	2	75	+	
77	+		81	+	$5\frac{17}{18}$	68	−	$6\frac{4}{5}$
75	−	1	78	+	2	71	−	3
74	−	2	76	+		77	+	2
72	−	4	73	−	2	76	+	1
82	+	5	77	+	1	72	−	2
70	−	6	70	−	5	78	+	3
76	−		73	−	2	70	−	4
76	−		74	−	1	72	−	2
72	−	4	75	−		83	+	8
82	+	5	74	−	1	69	−	5
75	−	1	73	−	2	73	−	1
82	+	5	76	+		79	+	4
80	+	3	81	+	5	75	+	
73	−	3	75	−		75	+	
79	+	2	73	−	2	76	+	1
73	−	3	76	+		68	−	6
74	−	2				76	+	1
80	+	3	18)1351		18)$39\frac{2}{9}$	80	+	5
20)1527		20)$76\frac{2}{5}$	Average = $75\frac{1}{18}$		M.V. = $2\frac{29}{162}$	20)1496		20)$70\frac{4}{5}$
Average = $76\frac{7}{20}$		M.V. = $3\frac{32}{100}$				Average = $74\frac{4}{5}$		M.V. = $3\frac{54}{100}$

Table III contains the records of the estimations of observer K. In the first column the stimulus intensity is stated; in the second, the number of separate judgments that the subject made on the intervals; the third contains the length of the normal interval in terms of a thousandth of a second, the fourth, the length of the average estimation of that normal, also in terms of a thousandth of a second. The fifth column contains the constant errors for each series, the sixth, the mean variations, and the seventh, the relation of the mean variation to the normal in percentages. In order to show exactly the average deviation from a perfect equality in percentage of the mean variations, and hence the deviation from the absolute validity of the law, the mean variation of those percentages is worked out and placed in the last column in each of the divisions of the tables. The summary of results contained in these tables represents practically the whole of the estimations made, and not a few of the best, or those which best suited the theory of the experimenter.

Table III is divided into three divisions. The first division represents results of experiments made during the academic year of 1897-98. These experiments were made for the double purpose of testing Weber's law, and also to examine the influence of a change of intensity in the limiting impressions which marked the beginning and close of the intervals. In them an auditory stimulus was used. The second division records experiments similar to the first set, except that no change in the intensity of the limiting impressions was made, and these experiments were made in the following academic year, 1898-99. The third division contains the estimations of intervals of much greater length, and in these the limiting impressions were made by visual stimulation, instead of auditory as in the former series. The records in the table are arranged with the interval increasing constantly from the top to the bottom of the column, so that in reading down the column of the constant error the point of maximal accuracy may be easily noted where the constant error is smallest and just where the plus error of the short interval changes into the minus error of the long interval. That point seems to be what is called above the unit of time, and by this arrangement its relative length for the different subjects may be easily seen.

Table IV is a similar record of the estimations made by W. They were made in the same years and under circumstances similar to those which obtained in the case of K.

Table v differs from the above two only in that the first division was more comprehensive in the case of S., so that the second division which contained a number of series of experiments on approximately the same intervals as division one, in the above tables, was omitted in his case.

Table vi is a set of trials made by B. on the short intervals only, viz., from 99σ to 254σ.

The gentlemen who acted as subjects in these experiments, represented respectively as K., S., W., and B., are Professor Kirschmann, Mr. M. A. Shaw, Mr. F. S. Wrinch, and Mr. S. Blumberger. Here we wish to acknowledge with gratitude our indebtedness to Professor Kirschmann, under whose direction our work was done, for the generous and learned assistance tendered us, both in our study of the literature of the subject and in the conduct of the experiments.

TABLE III.—Observer K.

Division 1.

Stimulus Intensity.	Number of trials.	Normal.	Average estimation.	C. E.	M. V.	% of M. V.	M. V. of the %'s of the M. V's.
Low	120	93	113	+20	4.5	4.8	0.82%
High..........	120	93	114	+21	5.1	5.3	
Low	60	101	117	+16	3.6	4.8	
High..........	60	101	114	+13	4.8	4.5	
Low	60	125	128	+ 3	4.5	3.6	
High..........	60	125	135	+10	4.5	3.6	
Low	60	150	152	+ 2	5.1	3.4	
High..........	60	150	150	....	7.9	5.2	
Low	60	187	189	+ 2	5.2	2.7	
High..........	60	187	190	+ 3	5.8	3.1	

Division 2.

Number of trials.	Normal.	Average estimation.	C. E.	M. V.	% of M. V.	M. V. of the %'s of the M. V's.
90	97	122	+25	5.10	5.28	0.81%
120	111	126	+15	5.60	5.05	
90	132	139	+ 6	4.96	3.75	
90	156	158	+ 2	6.49	4.19	
90	189	185	+ 4.52	6.48	3.41	
90	225	257	+ 5.05	5.58	2.48	

Division 3.

The following trials were made during the academic year 1896–97, on intervals of increasing length :

Number of trials.	Normal.	Average estimation.	C. E.	M. V.	% of M. V.	M. V. of the °/o's of the M. V's.
60	503	491	−12	15	3.11	
60	750	752	+ 2	31	4.15	0.99%
40	1500	1331	− 1.69	48	3.46	6.67%
60	2950	2802	− 1.48	183	6.21	
40	4600	5363	+ 7.63	803	17.46	
20	9050	10785	+17.55	1905	21.016	

NOTE.—In the estimations of the two longest intervals, viz., 4600σ and 9050σ, records of which appear in the last two lines of the table above, the mean variations are abnormally large, compared with estimations of the same length of interval made by the other two subjects at the same time ; and also compared with estimations made by the same subject at a later date. These later estimations were made on long intervals divided up into unequal rhythmical divisions, and hence they are not stated here.* We give in the last column of this division the mean variation of the percentages of the mean variations of the first four intervals, which we regard as normal, in addition to the mean variation of the whole six intervals, which we regard as abnormal.

TABLE IV.—Observer W.

Division 1.

Stimulus Intensity.	Number of trials.	Normal.	Average estimation.	C. E.	M. V.	% of M. V.	M. V. of the °/o's of the M. V's.
Low	60	101	125	+24	7.8	7.2	
High..........	60	101	121	+20	6.	5.5	
Low	40	125	129	+ 4	6.1	4.8	
High..........	40	125	127	+ 2	6.5	5.2	
Low	40	150	167	+17	5.4	3.6	0.92%
High..........	40	150	159	+ 9	5.9	3.9	0.72%
Low	60	187	180	− 7	7.5	4.	
High..........	60	187	180	− 7	10.9	5.8	

* See below, p. 47 *et seq.*

Division 2.

As in the previous table the above intervals were also investigated again in the next year, with results as follows :

Number of trials.	Normal.	Average estimation.	C. E.	M. V.	% of M. V.	M. V. of the %'s of the M. V's.
150	98	131	+33	8.38	8.59	1.51%
90	108	136	+28	8.67	8.03	
90	128	149	+21	7.93	6.19	0.67%*
90	157	149	− 8	5.79	3.69	
180	197	181	−16	9.71	4.93	
90	250	248	− 2	12.75	5.1	

* In Division 2 the first two intervals, i.e., 98σ and 108σ, are regarded as abnormal (see below). On this account we have given the mean variation of the percentages of the last four intervals, in which the recording was satisfactory, as well as that of the whole six.

Division 3.

The following trials were made during the academic year of 1896-97 on longer intervals :

Number of trials.	Normal.	Average estimation.	C. E.	M. V.	% of M. V.	M. V. of the %'s of the M. V's.
60	500	512	+ 12	34	4.48	0.727%
40	750	730	− 20	49	3.89	
40	1490	1414	− 76	47	2.98	
40	3030	2895	−135	142	4.47	
40	4500	4454	− 46	102	2.28	
20	9150	9825	+675	376	4.02	

TABLE V.—Observer S.

Division 1.

Stimulus Intensity.	Number of trials.	Normal.	Average estimation.	C. E.	M. V.	% of M. V.	M. V. of the %'s of the M. V's.
Low	180	107	141	+34	5.2	4.8	0.62%
High..........	180	107	140	+33	4.3	4.	
Low	180	125	152	+27	4.2	3.4	
High..........	180	125	150	+25	5.1	4.1	
Low	80	150	167	+17	6.8	4.5	
High..........	80	150	176	+26	6.	4.	
Low	120	187	188	+1	7.7	4.1	
High..........	120	187	186	−1	9.6	5.1	
Low	120	214	213	−1	6.3	2.5	
High..........	120	214	212	−2	6.7	2.8	

Division 2.

Number of trials.	Normal.	Average estimation.	C. E.	M. V.	% of M. V.	M. V. of the %'s of the M. V's.
60	500	497	− 3	32.	6.4	1.5%
60	745	728	− 17	39.8	5.2	
40	1505	1251	−254	74.4	4.9	
40	2920	3271	+351	238.8	8.1	
20	4500	4444	− 56	351.4	7.7	
20	8870	7884	−986	633.4	7.5	

TABLE VI.—Observer B.

Number of trials.	Normal.	Average estimation.	C. E.	M. V.	% of M. V.	M. V. of the %'s of the M. V's.
90	99	151	+52	8.41	8.45	1.73%
90	109	158	+49	7.25	6.63	1.34%
90	128	169	+41	8.07	6.29	
90	153	164	+11	6.65	4.34	
90	192	195	+ 3	7.3	3.71	
90	254	249	− 5	7.42	2.95	

Subject B. also had difficulty in registering the shortest intervals as fast as he heard them. On this account in the last column of his records the mean variation of the percentages of the last five intervals is given as well as that for the whole six.

We proceed now to the results indicated by the above tables. The effect produced by changing the intensity of the limiting impression has already been noted. In the trials made by K. the law of Weber holds very closely throughout all the intervals investigated except the last two (Table III., Div. 3). In these two intervals, 4600σ and 9050σ, the percentage of the mean variation is about four times larger than the average percentage for the other intervals, and the constant error is proportionately large, and is plus, where, according to the tendency shown in his other judgments and also in the judgments of other subjects on this same interval, it should be minus. This anomaly in the case of the two longest intervals very probably arose from the fact that the subject, K., in his anxiety to exclude any external factor which might influence his estimation of the interval, interrupted the regularity of the organic functions which naturally make up, at least in part, the content of longer intervals. Hence in attempting to exclude this content, which normally has its part in the estimation of longer intervals, his judgments became abnormal, and in addition to this source of error, which in a measure accounts for the irregularity, the subject was somewhat distracted by the pressure of other duties[1] at the time when these experiments were made. Leaving out those two intervals then, we find that the mean variation of the whole of the percentages of the mean variations for the various intervals in all three divisions of the trials made by K. amounts to less than one, the mean variation for the first division being 0.82, for the second 0.81, and for the third, apart from the two last intervals, only 0.99. The very close uniformity in the results of the different divisions of K.'s estimations has greater significance from the fact that the experiments were made in successive years—division three in 1896-97, division one in 1897-98, and division two in 1898-99. This circumstance, together with the fact that the experiments of division three were made with a visual stimulation and those of divisions one and two with auditory stimuli, and also the large number of judgments, speak very conclusively for the validity of Weber's law.

In the shorter intervals K. uniformly reproduces the comparative interval longer than the normal. Up to 225σ there are none but plus signs in the constant errors. Above that, the intervals, with the exception mentioned above, are almost uniformly judged shorter. At the position where the constant error changes from plus to minus, the percentage of the mean variation is smallest, though in K.'s results there is no great variation in the percentages of the mean variation. K.'s unit of time lies probably not much above 225σ, since at 503σ the constant error has changed very considerably from plus to minus.

The testimony of W. does not differ much from that of K. In the second division of Table IV., in the first two intervals, as well as in the first interval of division one of the same Table, W. was conscious of being unable to react fast enough to record the intervals as he heard them. This accounts for the marked and abnormal increase in the percentage of the mean variation for

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[1]Annual Examinations.

these intervals. Apart from these shortest intervals, the mean variation of the percentages of the mean variations differs very slightly in the three groups, being for the first division 0.72, for the second 0.67, and for the third 0.72. These are even more uniform than the results of the trials of K., and the amount of the variation in the percentages is smaller. The circumstances under which the experiments were made were similar in the case of both subjects, the different divisions being made in different years, and one with visual stimulation, the other with auditory. Hence in the results of W.'s estimations the validity of Weber's law is more accurately established than in those of K., although the absolute difference in the two results is very small, so that their testimony is practically at one. The sign of the constant error changes from plus to minus at about the same position in both the groups of experiments, of which one was made in 1897-98, the other in 1898-99. The comparative interval is reproduced longer almost without exception up to a normal of 150σ, above that with a couple of exceptions it is reproduced shorter. The mean variation also reaches its minimum at the same point, which being the point of greatest accuracy in judgment indicates the length of W.'s unit of time. This unit for W. is much shorter than for K., and much shorter than the usual point of maximal accuracy, which lies between 200σ and 300σ. But the results of our experiments do not justify the theory that the point of maximal accuracy in the estimation of intervals is at all uniform; on the contrary it appeared to differ considerably in different individuals.

In Table v., which records the judgments made by S., exceptions similar to those noted above, in the longest intervals of K.'s work and in the shortest of W.'s, do not appear. The mean variation of the percentages of the mean variations of the several intervals tested in division one is only 0.62, an amount smaller than in the case of either K. or W., and the work in that division was very comprehensive, comprising over 1350 separate estimations. In the second division the variation is a little larger though still comparatively small, being only 1.5. Hence S.'s estimations confirm the testimony of the two previous subjects to the validity of the psychophysical law. S.'s time unit is quite definitely marked. It lies between the units of the previous two subjects, a little longer than that of W. and much shorter than K.'s, at about 187σ. There are almost no exceptions to the longer reproductions of the comparative in the intervals shorter than 187σ, and at only one point in the longer intervals is the average reproduction in the comparative not shorter than the normal.

B. did not complete the whole series of intervals which were investigated by the other three subjects. But in the trials made on shorter intervals, recorded in Table VI., his results do not vary much from the testimony of the others. He also, as in th case of W., found difficulty in recording the shortest interval viz., 99σ, as fast as he heard the limiting impressions, and hence we omit the record of that interval in calculating the mean variation of the percentages of the mean variations. The variation in the percentages of B.'s trials is a little larger than for the other subjects, but it is still comparatively small and points in the same direction, towards the validity of Weber's law. So far as relates to the intervals with which B. worked, his time unit is quite definitely marked. It lies at about 200σ, longer than that of W. and S., but still considerably shorter than K.'s.

To sum up the results of our experiments in brief—the testimony of the whole four subjects inclines clearly towards a confirmation of the validity of the law of Weber, at least within the limits covered by the intervals investigated, viz., from 93σ up to 9150σ. The mean variation of the percentages of the mean variations for all the intervals in the 1600 judgments made by K. is only 0.87, in the 1330 judgments made by W. it is only 0.70, in the 1600 judgments made by S. it is only 1.06, and in the 540 judgments made by B. it is 1.34. As to the comprehensiveness of the ground covered by the intervals tested, our apparatus, as noted above, is arranged so that with the least possible exertion the subject records his judgments; and since with this delicately adjusted apparatus the subjects found it difficult or impossible to record the smallest intervals as quickly as they heard the limiting impressions, we may justly assume that the series starts with the shortest intervals for which it is possible to record judgments. Also, in the longest intervals of the series a point is reached where the judgment depends very largely on the content of the interval; hence if longer intervals were tested, the same principle would be involved in their estimation, and the same results would thus naturally ensue. Thus we think that the results of the investigation of the above series of intervals practically cover the whole scope of time estimation in as far as it is accessible to a direct test of Weber's law. With each of the subjects the point of maximal accuracy of judgment, or the time unit, is quite definitely marked, and does not vary materially in the experiments made by the same subject in different years. But this point differs in its position for each of the subjects; for K. it was an interval of about 300σ, for B. about 250σ, for S. 200σ, and for W. about 175σ. Thus the time unit differs in length for different individuals, but it is a definitely marked interval in each case.

V.

REPRODUCTION OF COMPLEX RHYTHMICALLY ARRANGED GROUPS OF INTERVALS.

A further series of experiments was undertaken to investigate the degree of accuracy that obtains in the reproduction of complex rhythmically arranged groups of intervals of unequal length. This series was also undertaken in part for the purpose of testing further Weber's law, by applying it to somewhat varied conditions of reproduction. In these experiments, instead of using as stimulus a uniform series of intervals, limited by visual or auditory impressions, as was done in the previous experiments, an unequal series was used. In the first two groups for each of the three subjects the stimulus was given by clicks made as before, but in this case one of the intervals between the clicks was twice as long as each of the others. In the remaining groups of intervals, which were of increasing complexity, no mechanical stimulation was given. Each subject chose some grouping which was familiar to him, and reproduced it a certain number of times, making the records on the kymograph as described above for intervals limited by auditory impressions. The groups of intervals were chosen quite arbitrarily; whatever was familiar to the subject and sufficiently complex to satisfy the requirements of the experiment was allowed, *e.g.*, the rhythm of the beats of a drum, a couple of bars of some popular air, or a group made up impromptu and easily retained in consciousness.

Before inserting the tables which contain the summed results of the experiments, one sheet of the readings in full of such a series is given. A cut of the records on one of the sheets made in this series, taken from the drum of the kymograph, is also included in Fig. 3 above.

Table VII. contains the records of judgments made by K. It is almost self-explanatory. There are eight groups of intervals, the simplest containing only three intervals, the most complex nineteen. The first line in each group shows the number of intervals and the number of judgments made on each. The second gives the average length, in order, of each of the respective intervals of the group. The third line gives the amount of the mean variation of each of the intervals, and the fourth the relation in percentages which the mean variations bear to the length of the intervals. This last is included for convenience in comparing the relative accuracy of the judgments on the different lengths of intervals. In the last line is given the average of the total duration of all the intervals of the group summed, the total mean variation, and the percentage of the mean variation. Tables VIII. and IX. are similarly arranged records of the judgments made by the other two subjects, W. and B. The length of all the intervals in these, as in the other tables, is stated in terms of one thousandth of a second.

(One sheet of the readings in full.)

Observer K.

Stimulation, twenty intervals from the "Boccaccio March."

435	340	115	250	255	455	440	315	135	775
445	310	125	230	240	435	435	320	145	760
435	310	120	240	240	435	450	325	140	785
435	320	110	235	245	465	420	300	130	830
430	330	120	235	220	435	435	300	120	775
445	320	110	245	240	435	435	310	140	785
455	340	95	255	240	435	440	325	125	785
440	325	115	255	230	440	440	320	145	840
445	325	120	250	235	450	435	350	125	780
465	330	125	265	210	435	420	330	130	810
10)4430	3250	1155	2460	2355	4430	4355	3195	1335	7925
443.	325.	115.5	246.	235.5	443.	435.5	319.5	133.5	792.5
M. V. = 8.	8.	6.5	9.	9.4	10.2	5.6	5.6	7.5	20.5
% 1.8	2.5	5.6	3.6	3.9	2.2	1.2	1.7	5.6	3.5

Total duration of the 10 intervals = 3489σ.

Total mean variation = 90.3 = 2.5%.

Mean variation of the percentages of the M. V.'s = 1.25.

Table VII.—Observer K.

Group 1 :—No. of intervals=3. No. of judgments on each=13.

Average length......	379	192	182
Mean variation......	11.98	8.28	4.08
% of M. V..........	3.1	4.3	2.2

Total duration of group 752σ. Total M. V. 24.26=3.2%.

Group 2 :—No. of intervals=3. No. of judgments on each=14.

Average length......	402	189	189
Mean variation......	16.53	5.71	7.55
% of M. V..........	4.2	3	4

Total duration of group 772σ. Total M. V. 29.79=3.8%

Group 3 :—No. of intervals=5. No. of judgments on each=17.

Average length......	284	288	130	137	352
Mean variation......	13.04	12.56	10.59	14.58	18.37
% of M. V...........	4.6	4.3	8.1	10.6	5.2

Total duration of group 1192σ. Total M.V. 69.15=5.8%.

Group 4 :—No. of intervals=5. No. of judgments on each=22.

Average length......	548	546	522	267	264
Mean variation......	24.21	20.45	16.05	9.77	10.2
% of M. V..........	4.4	3.7	3.1	3.6	3.8

Total duration of group 2103σ. Total M. V. 80.6=3.8%.

Group 5 :—No. of intervals=7. No. of judgments on each=15.

Average length......	374	128	287	258	263	263	544
Mean variation......	12.8	15.24	16.44	10.8	10.93	8.91	30.13
% of M. V..........	3.1	4.3	2.2	4.2	4.1	3.4	5.5

Total duration of group 2117σ. Total M. V. 105.27=4.9%.

Group 6 :—No of intervals=8. No. of judgments on each=12.

Average length......	212	560	214	617	203	235	218	623
Mean variation......	15	38.33	12.43	28.89	15.27	12.15	11.25	31.38
% of M. V..........	7.1	6.8	5.9	4.7	7.4	5.2	5.1	5.1

Total duration of group 2886σ. Total M. V. 164.72=5.7%.

Group 7 : No. of intervals=10. No. of judgments on each=10.

Average length..	443	325	115	246	235	443	435	319	133	792
Mean variation..	8	8	6.5	9	9.4	10.2	5.6	5.6	7.5	20.5
% of M. V......	1.8	2.5	5.6	3.6	3.9	2.2	1.2	1.7	5.6	3.5

Total duration of group 3489σ. Total M. V. 90.3=2.5%.

Group 8 :—No. of intervals=19. No. of judgments on each=6.

Average length.....	287	311	131	130	328	291	286	121	128	
Mean variation.....	10.55	22.77	17.77	11.11	25	11.11	10.55	5	11.5	
% of M. V..........	3.7	7.3	13.6	3.8	7.5	3.8	3.7	4.1	10	
Average length..	339	288	297	284	294	294	286	127	130	337
Mean variation..	12.22	5.55	7.5	9.66	6.5	5.55	13.88	4.15	5	10
% of M. V.......	3.6	1.9	2.5	3.4	2.5	1.9	4.8	3.3	3.8	3

Total duration of group 4697σ. Total M. V. 207.42=4.4%.

Table VIII.—Observer W.

Group 1 :—No. of intervals=3. No. of judgments on each=16.

Average length......	181	198	414
Mean variation......	3.48	11.72	30.03
% of M. V..........	1.9	5.9	7.3

Total duration of group 789σ. Total M. V. 45.23=5.7%.

Group 2 :—No. of intervals=3. No. of judgments on each=18.

Average length......	191	197	359
Mean variation......	9.16	7.22	27.46
% of M. V..........	4.8	3.6	7.6

Total duration of group 746σ. Total M. V. 43.85=5.8%.

Group 3 :—Number of intervals=5. No. of judgments on each=15.

Average length......	557	601	564	285	308
Mean variation......	18.75	22.80	27.15	13.02	8.44
% of M. V..........	3.4	3.7	4.8	4.5	2.7

Total duration of group 2311σ. Total M. V. 90.16=3.9%.

Group 4 :—No. of intervals 7. No. of judgments on each=13.

Average length......	645	199	165	356	335	358	673
Mean variation......	21.14	10.94	7.39	17.57	8.16	12.25	28.58
% of M. V..........	3.3	5.5	4.4	4.9	2.4	3.4	4.2

Total duration of group 2733σ. Total M. V. 106.05=3.8%.

Group 5 :—No. of intervals=8. No. of judgments on each=12.

Average length......	388	199	180	393	387	378	405	696
Mean variation......	22.08	14.02	7.22	12.22	22.5	16.11	9.65	22.36
% of M. V..........	5.7	7.1	4	3.1	5.8	4.2	2.4	3.2

Total duration of group 3029σ. Total M. V. 126.17=4.1%.

Group 6 :—No. of intervals=8. No. of judgments on each=9.

Average length......	499	268	935	198	201	277	654	722
Mean variation......	24.56	16.42	51.11	4.07	9.01	14.56	31.23	23.70
% of M. V..........	4.9	6.1	5.4	2.1	4.4	5.2	4.7	3.2

Total duration of group 3752σ. Total M. V. 174.68=4.6%.

Group 7 :—No. of intervals = 10. No. of judgments on each = 7.

Average length..	667	473	223	597	427	264	688	175	214	932
Mean variation..	36.53	32.24	16.94	23.47	20.2	15.1	17.09	2.85	12.04	74.89
% of M. V......	5.5	6.8	7.3	3.9	4.7	5.7	2.5	1.6	5.6	8

Total duration of group 4660σ. Total M. V. 256.37 = 5.4%.

Group 8 :—No. of intervals = 15. No. of judgments on each = 8.

Average length.....	517	340	165	785	528	331	175	642	195
Mean variation	21.87	22.5	8.75	46.87	10	16.15	4.37	69.37	12.03
% of M. V..........	4.2	6.6	5.3	5.9	1.9	4.9	2.5	10.8	6.2
Average length.....	360	180	507	514	520	919			
Mean variation.....	6.25	6.25	13.12	14.53	17.5	27.5			
°/₀ of M. V..........	3.1	6.2	2.5	2.8	3.3	5.5			

Total duration of group 6683σ. Total M. V. 332.31 = 4.9%.

Table IX.—Observer B.

Group 1 :—No. of intervals = 3. No. of judgments on each = 21.

Average length......	373	198	178
Mean variation......	17.19	7.17	8.02
% of M. V..........	4.6	3.6	4.4

Total duration of group 751σ. Total M. V. 32.39 = 4.3%.

Group 2 :—No. of intervals = 3. No. of judgments on each = 22.

Average length......	330	187	170
Mean variation......	20.08	7.72	9.54
% of M. V..........	6.1	4.1	5.6

Total duration of group 693σ. Total M. V. 37.35 = 5.4%.

Group 3 :—No. of intervals = 5. No. of judgments on each = 16.

Average length......	527	471	252	254	500
Mean variation......	20.51	18.12	9.61	8.98	32.18
% of M. V...........	3.8	3.8	3.8	3.5	6.7

Total duration of group 2005σ. Total M. V. 89.41 = 4.5%.

Group 4 :—No. of intervals = 7. No. of judgments on each = 12.

Average length......	393	214	203	433	408	369	453
Mean variation......	25.41	26.18	31.04	29.16	29.58	22.91	33.12
% of M. V..........	6.4	12.2	15.2	6.7	7.2	6.1	7.3

Total duration of group 2476σ. Total M. V. 197.42 = 7.9%.

Group 5 :—No. of intervals = 8. No. of judgments on each = 15.

Average length......	400	290	530	224	198	228	557	588
Mean variation......	22.76	19.33	43.33	9.86	12	11.82	39.77	32.13
% of M. V..........	5.6	6.6	8.1	4.5	6	5.2	7.1	5.4

Total duration of group 2616σ. Total M. V. 192.52 = 7.3%.

Group 6:—No. of intervals = 15. No. of judgments on each = 7.

Average length......	403	312	178	706	391	313	177	554	188
Mean variation......	16.53	14.69	9.18	47.96	18.98	20.61	13.06	26.32	13.87
% of M. V..........	4.1	4.7	5.1	6.7	4.8	6.3	7.3	4.7	7.3
Average length....	317	175	403	414	424	661			
Mean variation....	18.16	8.57	14.49	23.47	24.39	47.22			
°/₀ M. V..........	5.7	4.9	3.6	5.6	5.8	7.1			

Total duration of group 5625σ. Total M. V. 318.02 = 5.6%.

To turn now to the results of the experiments. We have seen that in reproducing one uniform interval in a successive series, the average mean variation in the whole of K.'s judgments was about 4.1 per cent., and that there was not much deviation from this mean in intervals of different length (cf. Table III.). In the present experiments, where instead of a series of reproductions of one interval of uniform length a series of groups of intervals of unequal length rhythmically arranged is reproduced, the average mean variation for all the groups and intervals is 4.2 per cent., which is only one-tenth per cent. greater than for the simple interval. Although there is some deviation from this average in individual intervals, still in the averages for the whole groups the mean variation is very uniform. This is another illustration of a phenomenon noted by Pringle in experimental investigation of poetical metre,[1] where there was a distinct tendency to complete a defective line, so as to make a time unit for the respective lines in a stanza. The phenomenon in these experiments shows itself in the fact that although there is a deviation within a narrow limit among the individual intervals of the more complicated groups, there is very little deviation in the average mean variation of each complete group.

The relation of the different lengths of intervals to one another throughout the judgments of the different groups is very constant, there being no distinct or regular falling off in the accuracy from the first reproduction of the group to the last. This is well illustrated in the detailed readings of the sheet on page 46, which is a fair average representative of the other sheets. There is if anything rather an increase in accuracy than a falling off; in the above sheet the last two reproductions of the group are a little more constant, and a little nearer the average, than the first two. We do not wish to infer from this that there is

[1]The records of his work are preserved in the archives of the Psychological Laboratory, University of Toronto.

a constant increase in accuracy in the reproduction of the groups, but that within certain limits they are comparatively uniform.

Referring again to the above sheet we find in the ten intervals four distinct types. The first is about 440σ in length, the second about 320σ, the third about 120σ, and the fourth about 240σ. The last interval in the group, averaging 792σ, is probably in part composed of the pause at the end of the line, and thus it does not represent a definite length of note used in the composition. In the above four typical lengths of intervals, there is not the definite relation of one, a half, a fourth, and an eight, as in written musical compositions. The shortest interval, 120σ, is about half of the next shortest, but it is not a third of the next, nor a fourth of the longest. The subject who made the judgments in the above sheet is neither a true musical artist, nor a mechanical musician who would be dominated by the measure written on the sheet of music before him, but one who thoroughly appreciates melody and rhythm. Our experiments do not justify any definite conclusion as to whether the relative lengths of intervals in musical rhythm, as represented in the interpretations of the true artist or of the appreciative reproducer, are the same as are represented on the written sheet. Such a conclusion would need the support of a special set of investigations. But the indications from our own experiments are that the relative length of musical notes in the production of a true musician is not in the exact ratio of one to a half, a quarter, an eighth, and so on. A further point illustrated by the above sheet is the indifference of the length of the interval to the pitch of the tones composing the melody. The intervals recorded on this sheet are intended to represent twenty intervals from the "Boccaccio March;" the rhythm in the first ten is similar to that of the second ten, but the pitch of the tones is quite different. In the records the second group of ten is placed directly under the first, and in that order through all the judgments made; there is no noticeable regular difference between the lengths of the intervals in the respective groups, notwithstanding the great difference of pitch.

In the first two groups of the table, containing three intervals each, a mechanical auditory stimulation was used to give the subject the rhythm, but this stimulation ceased before he began to record. In the physical stimulation one interval was equal in length to the sum of the other two, and it was repeated in a regular succession so that either short or long interval might come first. The judgments of K. in the first group come definitely in the rhythm of the dactyl, *i.e.*, the long interval comes first, retaining its relation as almost exactly equal to the sum of the other two, the second interval is lengthened at the expense of the third, being 10σ longer, and the third is the shortest. This is the normal relation of the intervals of the dactyl, the second interval being longer than the third, though the relative lengths be not quite those given above.[1] The second group does not fall quite so definitely into dactylic rhythm, since on the average the two shorter intervals are exactly equal, but in the individual judgments seven out of fourteen are in the dactylic rhythm and two have the two shorter intervals equal. So that with K. there is a marked tendency to throw groups of three intervals, one long and two short, into the rhythm of the dactylic poetic foot.

The other Tables, VIII. and IX., very generally confirm the tendencies noted in connection with Table VII., with the exception of the last point, in reference to the dactylic form of the grouping of the three intervals. In regard to the comparative accuracy of the successive reproduction of the single uniform interval and of the groups of unequal rhythmically arranged intervals, with K. there was an average difference of one-tenth per cent. in favour of the accuracy of the judgment of the single interval; the average percentage of the mean variation for the latter was 4.1 per cent., and for the groups 4.2 per cent. For W. the average difference is three-tenths per cent., being in the one case 4.4 per cent., and in the other 4.7 per cent. The difference for observer B. is greater; for the single interval his average was 4.8, and for the groups 5.8. But the work of B. was not so comprehensive as that of the other two subjects, either in the number of judgments made or in the scope of the intervals tested, so that his testimony is not so reliable as theirs.

In the groups of three intervals, where one was equal in length to the sum of the other two, W.'s results differ from those of K. Instead of the dactylic rhythm, W. definitely relates them in the rhythm of the anapæstic foot. In the first group his

[1]Cf. Hurst and McKay, *Experiments on the Time Relations of Poetical Metres*, *infra.*

judgments are 181σ, 198σ and 414σ, and in the second 191σ, 197σ and 359σ. In both cases the longer of the two shorter intervals comes before the longest instead of after. B. again in these groups reverts to the same order that was noted in K.'s judgments, but they are more pronounced in their dactylic rhythm than were his, since in both of B.'s groups the longer of the two shorter intervals comes after the longest, and is about 20σ longer than the shortest. What is involved for the question of poetic metre in this fact, that one subject hears a certain series of sounds in the order of the anapæst, and that another hears the same series in the order of a dactyl, we leave for a more extended investigation of poetic rhythm.

The results in the reproduction of these grouped intervals do not vary much in their testimony to the validity of Weber's law from the results of the previous series of experiments. With the interval represented by the sum of each of the groups, they are not quite so uniform in K.'s judgments as in the previous experiments. But, in spite of little irregularities, for the whole of the percentages of the mean variations of the eight groups of K.'s judgments, the mean variation is only 1.14. For the eight groups of W.'s judgments the figures are just as uniform as in his earlier experiments, the mean variation of the percentages of the eight groups being only 0.67. For B. it was a little larger, 1.12. In the individual intervals of the several groups of each subject there were some cases of irregularity which would seem to vary the testimony, but these are so few as to make it very probable that they have their source in some irregularity in recording the judgments, or in some slight confusion on the part of the subject. Generally the percentages of the mean variation for the different lengths of intervals in the groups vary little, the average length of deviation in many of them being less than one per cent. Thus in this series of experiments, as well as in the previous one, the results point quite definitely towards the validity in time estimation of the relation stated in Weber's law.

EXPERIMENTS ON TIME RELATIONS OF POETICAL METRES.

BY

A. S. HURST, B.A., AND JOHN McKAY, B A.

(UTILIZING ALSO EXPERIMENTS BY G. C. F. PRINGLE, B.A.)

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Lotze's statement that "the immediate experience of rhythm is as easy as its psychological explanation is difficult", while true for rhythm in general, is doubly applicable to that province of rhythm which has to do with poetic metres. Nearly everybody when speaking or learning verses enjoys the rhythmic effect; but like the balancing in tight-rope walking, bicycle riding, or even more ordinary periodic activities of the body, such as walking or running, we produce it and perceive it, but we do not know how it arises. This is partly due to the fact that, like the activities mentioned, poetic rhythm rests partly on reflex processes and is therefore excluded from direct psychological interpretation; but also partly because the actual facts are not sufficiently and unambiguously analyzed. Where attempts at scientific analysis are made, they are, as Prof. Meumann[1] points out, by no means free from that unscientific transference of terms from one realm to another in which they are inadequate or have no meaning at all.

We believe, with Professor Kirschmann, that a thing, a circumstance, etc., is not known or defined until it is expressed in terms of simple states of consciousness. Thus, for instance, there is no determination of what metallic lustre really is until it has been shown what combinations of the five variables of which sensations of sight are capable (intensity, colour tone, saturation, time relations and space relations) must occur in order to produce it. Similarly, according to Professor Kirschmann, the efforts of the phonetists graphically to trace the movements of the vocal organs must be regarded as comparatively futile or at least as by no means conclusive for the analysis of the elements of speech,

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[1] *Philosophische Studien*, Vol. X., p. 266

since these movements are not the sounds heard, and the same sounds can, as in the case of the ventriloquist, be produced without such movements. What has to be analyzed is the perceived fact and not the means by which that is brought about, or the accompanying circumstances, which may be merely accessory.

The same doctrine holds for the analysis of all rhythmical phenomena. Without reference to the genetical and physiological theories about the causes of rhythm, and without direct relation to the æsthetic concomitants, we should first make a purely psychological analysis of the rhythmical phenomena themselves. In the case of poetical rhythm, the analysis hitherto made has been most superficial and insufficient. The only work which discusses the problem on a really scientific psychological basis is the excellent monograph of Professor Meumann.[1] In it, he treats of the general problem of rhythm, and examines the various theories genetic, teleologic, and purely æsthetic. Dealing with poetic metres in spoken verse, he claims that the rhythm is not merely in the language but in the thoughts, and that the total effect is due to a modification of the verse rhythm by the thought rhythm. This statement does not seem to be adequate, as in nearly all cases of spoken verse the verse rhythm gives way entirely to the thought rhythm. He says—" Regular scansion of verses is offensive because it accentuates the insignificant as well as the significant. " While this is true for the reading of verses, yet the regular scansion is of the utmost importance as a mechanical guide to the construction of the verse, and it is only by such scansion that the time relations obtaining within the verse can be determined It is just in this field that the problem of time relations of poetical metres lies; and by an experimental examination of the scanned verse many of the underlying principles of poetic metres may be discovered.

Much of a theoretical nature has been written in regard to the time element in metre. Lanier[2] claims that the "long" and "short" units employed in classic poetry are inadequate to account for all the variations of time relations in the different metres, and he substitutes a notation based upon the musical notation. "The poet, after having clearly indicated this normal time value of each bar, may then go on to vary the individual

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[1] *Philosophische Studien*, Vol. X., p. 249 ff and p. 393 ff.

[2] *Science of English Verse*, Chap. III.

time values of the constituents in any given bar at pleasure . . . . . When the poet selects, *e.g.*, the two words "wistfully wandering" to begin his verse, he does exactly what the musician does in writing the figures $\frac{3}{8}$ at the beginning of a strain . . . . both announce to the reader (of the poem or of music) that the normal time value of each bar in that strain (of music or of verse) is to be three-eighth notes, with the first of each three accented." But the musician, having made this announcement, is at perfect liberty to vary the arrangement of that $\frac{3}{8}$ interval in as many ways as the nature of musical notes will permit. So also with the poet. His foot may be the iambus, *i.e.*, a $\frac{3}{8}$ measure, but he is at liberty to make that foot up of an eight hand a quarter syllable, or of three eighth syllables, or of two eighths and two sixteenths, etc. While this is a most ingenious theory, and one which might seem to simplify the subject of metres, it is unfortunately far from being due to the facts of the case. No doubt "long" and "short" units of measurement, in the relation of two to one, are not sufficient to account for the different time relations in English metres; but it is not true, as the results given below prove, that any such regular relations as that claimed in the above theory obtain among the different syllables which constitute feet in English metres. T. L. Bolton,[1] in his admirable discussion of the general subject of rhythm, devotes some space to the discussion of poetic metres, more especially from the theoretical standpoint, having reference principally to the origin of metre and its general relation to the verse. He emphasizes the importance of the time element in poetry. "Poetry has never lost the time element entirely, for accents that occur at irregular intervals could not have been but very displeasing, and they are now. It is reported of some of our modern poets, and especially of Tennyson, that they read their poems with the strictest observance, not only of the accents, but of the time; showing that they regarded the time element as of great importance." Bolton, however, does not attempt to investigate experimentally any of the problems which have to do directly with poetic metres. L. A. Schurman[2] has investigated the rhythmic relations of sentences in the prose writings of different authors, and he concludes that the length of

[1] *American Journal of Psychology*, Vol. VI., p. 145.

[2] *University Studies, Nebraska University*, Vol. I.

sentences in prose does not deviate from an average; but the only attempt which has so far been made to investigate scientifically the different phases of poetic metres is that of Brücke[1] described by Prof. Meumann in the article referred to above.

Brücke's experiments investigated time relations only. They were made on the old form of the chronograph. In his first set of trials, intervals were registered by beating the rhythm with the finger on a pointer which registered on the smoked surface; in the second, by means of a lever attached to the lower lip or the teeth. His conclusion is that, roughly speaking, the arses occur at equal distances, *i.e.*, that the feet in any given verse are almost equal. Meumann[2] points out that this form of the chronograph recorded only large intervals, and, therefore, was inadequate to such an investigation. With reference to the second form of the experiment a further objection holds. The results attained could have no direct bearing on the problem, as the phenomenon to be investigated was the rhythmic succession of sounds, not the movements supposed to produce it, as pointed out above, which might be merely accessory to it.

A modified form of Brücke's first method is, we believe, the correct one. By means of the improved form of the chronograph, described in the article of Messrs. Shaw and Wrinch, which allows of easy distinctions of at least one two-hundredths of a second, a series of experiments, bearing on the problem, has been conducted during the past two academic years in the psychological laboratory of the University of Toronto. Brücke measured from arsis to arsis, obviously having no other mark than the crests of his wave-like records, whilst in ours the beginning of each rhythmical reaction is sharply marked by an abrupt departure from the abscissa line. The series was begun by G. C. F. Pringle, B.A., and continued throughout the year 1897-98. Mr. Pringle's results, together with those which we have obtained during 1898-99, are embodied in this paper. The work has been done under the supervision of Professor Kirschmann, from whom we have obtained much valuable assistance. We are also indebted to Professor Alexander for interest manifested and suggestions made during the progress of the work.

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[1] Brücke's own works were not accessible to the writer.

[2] *Philosophische Studien*, Vol. X., p. 419.

It must be borne in mind that the problem which we have attempted to investigate has less to do with recited verse than with poetic metre considered as a regulative principle in the construction of the verse. Various theories have been advanced as to what is the main factor in poetic metres. Guest[1] takes no account of feet or measures of English verse, but lays great stress on accent. The character of the verse is determined, in his view, by the regular succession of accents. He recognizes three classes of lines, those that begin with an accented syllable, those that begin with one unaccented syllable, and those that begin with two unaccented syllables. He also recognizes varieties in these. If the line beginning with the accented syllable consists of a regular recurrence of accented and unaccented syllables, it constitutes one variety; if, however, two unaccented syllables occur between the accented syllables there are two different varieties, according as the two unaccented occur between the first and second accented syllables, or between the second and third, and so on throughout the line. This is nothing more than describing in a roundabout way the regular iambic, trochaic, dactylic and anapaestic feet from the point of view of accent only. For the line beginning with an accented syllable and consisting of a regular succession of accented and unaccented feet is nothing more than a series of trochees; so also the line beginning with an accented syllable and followed by two unaccented syllables, is nothing more than a line of dactyls, and that beginning with two unaccented syllables followed by an accented syllable, is an anapaestic line. As far as the accents are concerned, this is a correct description of the various metres; but we see no reason why the term feet should not be used, as the description which he gives of the line implies just such groups of syllables as are ordinarily classed under the term "feet." But, in so far as this description implies that the time relations of the different syllables play no part in the structure of the line, our results show it to be erroneous.

Gummere[2] says: "In the Germanic languages and in nearly all modern poetry, accent is made the principle of verse; we weigh our syllables; we ask how much force, not how much time

[1]See Bolton's article, *American Journal of Psychology*, Vol. VI., p. 145, and Guest's *History of English Rhythms*, p. 108, etc.

[2]*Handbook of Poetics*, p. 137.

they require. Meanwhile, we do not utterly refuse to recognize quantity as an element of verse, nor was classic poetry unfamiliar with accent. In the latter an ' ictus ' or stress fell upon the long syllable; in modern verse, while the main principle is the alternation of heavy and light syllables, we nevertheless admit quantity as a ' regulative ' element. It is a secondary factor of verse. . . . . . . Accent, then, is the chief factor of modern verse." He thus makes the time element secondary to that of accent, which he believes to be the main factor in modern verse. While this may seem to be true for spoken verse, it must be borne in mind that both the time and accentual rhythm are, to a great extent, ignored in reading verses for the sense alone, rhythmic effects being then largely due to the thought. Lotze has even said that time plays no rôle at all in the rhythm of verse. In metre, considered merely as a regulative principle, as in a scanned line, we believe with Meumann that time and accent each plays a leading part, and that to a certain extent each can replace the other.

Our experiments, as stated above, had to do only with the time relations in the scanned verse and were conducted as follows:—

I.—Poems representing each of the four ordinary metres (dactyl, anapaest, trochee, and iambus) were recited, rhythm being marked by the reciter striking the pointer of the chronograph in unison with the rhythm of the lines read; this recorded the lengths of the various syllables on the smoked paper of the cylinder. The poems in every case were scanned, not read in the ordinary manner, and were repeated inaudibly by the operator.

II.—The different metres were marked empty, *i. e.* without words being thought or said, attention being given to the regularity of the rhythm alone.

The following poems and parts of poems were examined:

I. Dactylic—

(a) Scott's *Pibroch of Donnel Dhu*, second stanza, first four lines.

(b) *Ibid*, first stanza, complete.

(c) Scott's "Hail to the Chief who in triumph advances."

(d) Hood's "One More Unfortunate," first two lines.

(e) *Pibroch of Donnel Dhu*, fifth stanza, first four lines.

(f) Boat-song in Scott's *Lady of the Lake*, first stanza, second, third and fourth lines.
(g) *Pibroch of Donnel Dhu*, fifth stanza, complete.
(h) "Down from the mountain and up from the plain."
(i) *Pibroch of Donnel Dhu*, fourth stanza, first six lines.
(j) "Merrily, merrily shall I live now."
(k) "Over the river and over the Dee."

II. Anapaestic—
(a) "And a single small cottage, a nest like a dove's,
The one only dwelling on earth that she loves."
(Wordsworth's *Reverie of Poor Susan*.)
(b) Wordsworth's *Reverie of Poor Susan*, first stanza.
(c) "From the centre all round to the sea,
I am lord of the fowl and the brute."
(Cowper's *Alexander Selkirk*.)
(d) Moore's "In the morning of life," first stanza.
(e) "And the stream will not flow and the hill will not rise,
And the colours have all passed away from her eyes."
(f) Poe's *Annabel Lee*, first stanza.
(g) "In the calm of the noontide, in sorrow's dread hour."
(h) "In a coign of a cliff between lowland and high."

III. Trochaic—
(a) "Ave Maria, maiden mild,
Listen to a maiden's prayer;
Thou canst hear though from the wild,
Thou canst help amid despair."
(Scott's *Lady of the Lake*.)
(b) Tennyson's *Ode to Queen Victoria*, beginning
"Fifty times the rose has flowered and faded,
Fifty times the golden harvest fallen
Since our Queen assumed the globe and sceptre."
(c) First three stanzas of *Locksley Hall*.

IV. Iambic—
(a) Scott's *Lord of the Isles*, Canto II., stanza 30.
(b) "The way was long, the wind was cold,
The minstrel was infirm and old."
(Scott's *Lay of the Last Minstrel*.)

Records on these selections were made as described above by Professor Kirschmann, Professor Alexander, Messrs. G. C. F. Pringle, B.A., F. S. Wrinch, M.A., G. A. Kingston, W. A. Wilson, A. S. Hurst, and J. McKay. The operator was asked to scan[1] the selections, recording the regular rhythm, as well as any irregularities which might occur. In nearly all the cases the words were placed above their corresponding time indications in the records, with the result shown that not only all syllables which fell into the regular metrical arrangement, but also any which were irregular were recorded. Thus it was demonstrated that the operator actually followed the movement of the verse and was not led away by the bare rhythm. The following example will serve as an illustration :

come	from	deep	glen	and	from	moun-tain		so	rock - y.	
34	18	17	33½	17	16	32	18	17½	27½	28½

The length of the last syllable of the line is due to the end pause, which is recorded along with the regular length of the syllable. Whatever pauses occurred in any of the lines employed were similarly recorded.

In each record, not less than ten feet, and in a great many instances twenty or more, were marked. These were then added together, the mean variation and relative percentages were calculated, and from the results the tables which are appended to this article were prepared. The tables are almost self-explanatory. The average feet are expressed in hundredths of a second, the number of feet recorded in each trial, the selection on which they were made, and the name of the operator are given. Many more records were made than appear in the tables ; those which presented great irregularities and those in which the mean variation was too large were rejected.

In the selection of verses, it was found very difficult to get good dactylic lines ; since there is an exceedingly small number of pure dactyls in English, and where dactylic verses do occur, they present many irregularities. The same thing was noticed about trochaic verses. Both dactylic and trochaic metres seem to be almost entirely confined to stirring pieces with rapid movement, such as Scott's war songs, etc. It was also found that in following a dactylic or trochaic movement, even when the selec-

[1] By scansion is meant reading so as to pay attention only to the rhythm.

tion given presented few irregularities, the operator frequently dropped into irregular forms, but in anapaestic and iambic movements it was very rarely that any such deviations occurred. This would seem to be due to the fact that the quieter states of consciousness may last for almost indefinite intervals, whereas the more intense states can be maintained only for a short time, and the dactylic movements representing such states are therefore not suited to being continued for any considerable time. Iambic and anapaestic metres are therefore better adapted to the more lengthy forms of poetry (especially dramatic), while dactylic and trochaic verses are suited only to poems in which a rousing, stirring effect with rapid movement is desired.

It is always claimed that there are no amphibrachic measures in the Germanic languages. Professor Kirschmann, nevertheless, has directed our attention to the fact that the celebrated ballad of Bürger, "Der Kaiser und der Abt," is of the purest amphibrachic metre, the thought rhythm and the metric rhythm faultlessly coinciding. If we accept this view and do not regard the poem, as is usually done, as written in dactyls with an up beat, then the stanza would take the following form :

⏑ — ⏑ ⏑ — ⏑ ⏑ — ⏑ ⏑ — ⏑
⏑ — ⏑ ⏑ — ⏑ ⏑ — ⏑ ⏑ — ⏑
⏑ — ⏑ ⏑ — ⏑ ⏑ — ⏑ ⏑ —
⏑ — ⏑ ⏑ — ⏑ ⏑ — ⏑ ⏑ —.

In order to ascertain the actual time relations of the metre of this peculiar poem a few trials were made in the same manner as above, the operator marking time with the finger, and at the same time reciting the poem, in this case aloud. The result is very peculiar. There is almost no indication of long and short syllables, save that a tendency is shown to speak the accented syllable a little shorter than the others. It would seem that accentuation, *i.e.*, pitch and intensity, alone makes up the rhythm here. The following are the time values of one stanza :

35, 31½, 37, 33½, 33, 33, 31, 34, 35, 33, 32, 36½,
33, 30, 33½, 35, 33, 37, 35, 35, 39, 33½, 31½, 37,
34, 30, 36, 34, 36, 37, 36, 33, 33, 33, 58½,
33½, 36, 35, 36½, 31, 35, 34½, 31, 32, 34, 71.

The average of two records made by Professor Kirschmann on the above selection, in each of which the entire stanza was repeated twice, giving twenty-eight three-syllable groups, was as follows:

I. 33.732, 32.357, 33.714, *i.e.*, 1 : .9692 : .9994.
II. 31.72, 29.8, 31.84, *i.e.*, 1 : .9394 : 1.0037.

We now come to the conclusions suggested by our results.

I. In any given verse, other things being equal, there is, roughly speaking, a uniform length of foot; this is a confirmation of Brücke's conclusion quoted above. A good illustration is supplied by the following selection from a record made by Professor Kirschmann on the first two lines of Wordsworth's "Reverie of Poor Susan":

22, 23, 39—21, 23, 42.5—20.5, 22, 39—20.5, 21, 40—
19, 20, 43.5—20, 20, 41—21.5, 21.5, 43—18.5, 20.5, 45.

Expressing each foot as a total the line would appear as follows: 84—86.5—81.5—81.5—82.5—81—86—84, or the following percentages: 1—1.029—.9702—.9702—.9821—.9642—1.0238—1. The mean variation of the eight feet is 1.75. When it is remembered that this means 1.75 hundredths of a second, or .0175 of a second, it will be seen that the average deviation from the regular foot length is exceedingly small. This result was confirmed throughout all our records. not only in those which appear in the following tables but in those also which were rejected, nearly one thousand different feet in all being represented.

II. The dactylic foot tends to be shorter than the anapaestic and the trochaic than the iambic. Owing to the stirring nature of the dactylic and trochaic metres the movement is more rapid, and consequently the foot interval becomes shorter. For example, the first five results by McKay in Table A, being added, give the following foot intervals: 85.625—86—88.35—92.20—81.55, or an average foot of 86.745 in the anapaestic movement. Comparing with these the first five records by the same operator in Table B, we find the foot lengths for the dactylic movement to be: 81.35—89.5—83.9—86.2—83.6, or an average of 84.91. In Table C, the first five iambic foot records give, when added, the following results: 59.95—64.55—62—64.7—63.5, or an average iambic foot of 62.91. Corresponding figures for Table D, similarly treated, are: 67.35—61.5—57—52—63.5, making the average trochaic foot to be 60.2.

This conclusion is further corroborated by reference to Tables E and F, which are records of pure metre. The first four results in Table E obtained by Hurst show the following foot intervals: 72.4—60.1—71.6—78.7, an average for the pure metre dactyl of 70.7. In Table F the first four results of the same operator give the figures 77.7—73.25—68.55—80.7, an average of 75.05 for the pure metre anapaest. Our results thus show conclusively that the average anapaest is longer than the dactyl, and the average iambus longer than the trochee.

III. A result was noticed in reference to the relation of pure and filled metres (*i.e.*, metres recorded simply as metres, and those recorded while scanning a line), which, while it does not appear from the tables, was yet very apparent in the preparation of the records. It seemed to be much more difficult to record a pure metre than one made by scanning a line of poetry, and very many of the results obtained had to be rejected because of their lack of any regularity. This goes to show that the conscious attempt to produce metre with the mind fixed on the rhythm alone is much less successful than the production of it when the attention is fixed on the regular recurrence of rhythmic intervals. This would seem in itself a sufficient refutation of Lotze's contention quoted above, that time plays no part in poetic rhythms; for it shows that the most favourable method for producing regular rhythmical effects is by the recurrence of units, each possessing a temporal value, and it is only when each of these units is consciously present that any sort of regular rhythmic effect is easily produced. Even when an attempt is made to produce any of the ordinary poetic metres without any line in mind, the beats of the finger on the pointer take on the nature of the temporal units which are given to be reproduced in the case of a scanned line; and our results indicate that it is only when there is a mental picture of each of these temporal units and their relations to one another in the group we call a foot that anything like a regular result is produced.

IV. On reference to Guest's description of verses quoted above, it will be seen that the criticism there made is substantiated by our results. More is implied in a poetic line than a mere succession of accented and unaccented syllables, however large a part this may play. The general results tabulated below show that real temporal differences exist among the different syllables constituting a line, which, although partly accounted for by the unconscious effect of the accent on the recording of the syllable, yet are not wholly explained thereby. Furthermore, it is also shown by these results that certain groups of syllables form organic wholes, the parts of which exercise a marked influence on each other. These groups are what we are accustomed to call feet. This will be still better shown by taking a typical line from one of the records. The line "And the stream will not flow and the hills will not rise," the first part of II (e) above, give the following result:

18, 20, 30—20, 20, 39—19½, 20, 38—20, 22, 42.

It will be easily seen that it is not a sufficient description of this line to say that it is a line beginning with two unaccented syllables, followed by an accented syllable, and so on throughout the line; for the syllables plainly fall into groups of three, bound together not merely by the accent but by temporal relations. Thus, it will be observed that the second syllable is longer than the first, the three forming a kind of scale, beginning with the first syllable and gradually ascending to the concluding long syllable. So also the temporal difference between the accented and unaccented syllables cannot be adequately described without noting the part played by time in the line.

V. Lanier, as indicated above, makes time the basis of rhythm, but uses a kind of notation corresponding to musical notation, in which he gives certain fixed relations to the elements constituting a foot, but results do not show any such regularity of relation among the syllables of a foot. We are warranted in making the general statement that an iambus consists of a short syllable followed a long, a trochee of a long followed by a short, a dactyl of a long followed by two short syllables, and an anapaest of two shorts followed by a long, yet with no fixed proportions between the syllables. In the iambi of Table C, the syllables are seen to have a proportion of about 1 : 2, and in the trochaic Table D of 1 to a little less than 1.5. The anapaests in Table A are about 1 : 1.1 : 2, and the dactyls in Table B about 1.6 : 1.1 : 1. These are averages of a large number of results, and the proportions of the syllables to one another admit of a wide range of variation without destruction of the essential character of the metre. Great flexibility is thereby given to these metres in

English, and a great variety of words of slightly different time values may be used. It is only when the proportions of these syllables to one another are reversed that an unpleasant effect occurs and the metre is vitiated. For example, in the above figures given for anapaest and dactyl, if the second short syllable were to be interchanged with the first, thus :

dactyl 1.6 : 1 : 1.1.
anapaest 1.1 : 1 : 2.

then the anapaestic or dactylic character of the respective feet would be lost. This is illustrated by one or two of the results embodied in Tables A and B.

VI. The question is raised by Bolton[1], " What is the inherent nature of a group in a rhythmical series, or what is the relation of the different syllables to one another, and what determines the length of it ? " but he makes no attempt to answer it. Meumann says[2] : " Dactylic lines may be read with anapaestic movements and vice versa," implying that there is no essential difference between a dactylic and an anapaestic line, excepting the fact that the dactylic line happens to begin with an accented syllable. Tables A and B show that it is by no means true that the only difference between the anapaestic and dactylic foot is the fact that the anapaestic has the accent and long syllable at the close and the dactylic at the beginning. There is a real internal difference in the foot. The anapaestic foot begins with two short syllables (the second of which is in every case longer than the first), followed by a long syllable ; the dactylic begins with a long syllable followed by two short ones, the first of which is longer than the second. Compare the line " Hail to the chief who in triumph advances," which gives the following results :

41, 21, 18½—40, 21½, 20—37, 22½, 17—51

with the anapaestic line " And the moon never beams without bringing me dreams," which gives :

16½, 21, 43—16½, 20, 44—17, 22, 45—17½, 19, 43.

If, now, a dactylic line differs from an anapaestic only in beginning with a long accented syllable, should this syllable be read alone, the rest of the movement will be the same as in the

[1] *American Journal of Psychology*, Vol. VI., p. 173.
[2] *Phil. Studien*, Vol. X., p. 416.

regular anapaestic line. Applying this to the above dactylic line we see that the movement is *entirely* different from that of the corresponding line of anapaests.

(41) 21, 18½, 40—21½, 20, 37—22½, 17, 51—
16½, 21, 43—16½, 20, 44—17, 22, 45, etc.

If we mark the shortest syllable (a), the second in length (b), and the long one (c), the changed dactylic verse here would give us a series:

bac, bac, bac, etc.,

while the regular anapaestic line is represented by the series:

abc, abc, abc, etc.

From this it is obvious that there is a real difference between a dactylic and an anapaestic line, a dactylic foot, according to the above notation, being represented by "cba" and an anapaestic by "abc." In each case the syllables seem to form an interconnected group, the determining factor in each being the long accented syllable (c). In the anapaest the first syllable is (a) and the second is (b), the latter being lengthened out by its proximity to the long accented syllable (c); in the dactyl, the last syllable is (a) and the first short syllable is (b), it also coming under the lengthening influence of (c). The anapaestic foot is a group in an ascending order of importance, the dactylic in a descending. This, so far as anapaestic and dactylic measures are concerned, answers the question asked above as to the inherent nature of the groups and the relation of the different syllables to one another. As the anapaestic and dactylic feet lend themselves better to experiment for this purpose, more time has been spent on their investigation than on the trochaic and iambic measures, and a larger number of trials made.

The differences are not so marked in the case of the iambic and trochaic metres, but Tables C and D seem to indicate the following differences:

(1) The foot in the iambic metre tends to be longer than in the trochaic, for the reason mentioned above, viz., the stirring, tripping nature of the trochee, as its name indicates.

(2) The proportion of the short syllable to the long in iambic metres is less than in trochaic, in the tables given the relation being about 1 : 2 for the iambus and about 1 : 1.5 for the

trochee. This is due to the nature of the foot. The foot beginning with an unaccented syllable and gradually rising into the accented syllable tends to prolong the latter, whereas the foot beginning with the accent tends to hurry over the accented syllable and complete the group, thus making the accented syllable in the trochee shorter in relation to the unaccented than is the case in the iambus.

VII. The notation commonly in use for the expression of the syllabic relations of anapaestic and dactylic feet is ⏑ ⏑ — and — ⏑ ⏑, indicating that the anapaestic foot consists of two short syllables, with no essential difference, followed by a long accented syllable, and the dactyl of a long syllable followed by two short syllables, between which also there is no essential difference. The results reached above would show that this notation is defective, and if the true nature of the foot is to be indicated some other notation must be adopted. As it is essential to any such notation that it should be simple and easily written, we suggest that the following notation, which indicates the difference in the short syllables and is as simple and easily written as the old, should be adopted, viz.:

anapaest: ⏑ ⌒ —.
dactyl: — ⌒ ⏑.

No change in the notation used for iambic and trochaic feet is advisable, as such notation is only of value to indicate relations within the foot and not absolute values, and this purpose is amply served by the notation now in use.

VIII. For the disputed amphibrachic metre the time relation seems to play a very unimportant part, the rhythm depending on accentuation alone. However, the experiments which we are able to contribute to this special division of the problem are not numerous and varied enough to warrant any definite conclusion. We have rather embodied them with our results as an incentive to further investigation.

Table A.—Anapaest.

Name.	Number of feet measured.	Verse.	Average length of syllables.			Relative percentage.			Mean Variation. (M.V.)		
			⏑	⌢	—	⏑	⌢	—	⏑	⌢	—
McKay	16	II. a.	24.375	23.375	37.875	1	.9589	1.5538	1.	.78125	1.875
Kirschmann	20	" b.	20.9	21.05	42.35	1	1.0071	2.0263	.96	1.1165	1.53
"	20	" c.	18.1	18.55	33.475	1	1.0243	1.8494	.0975	.7	2.19
Alexander	10	" d.	17.6	18.4	32.4	1	1.045	1.840	.82	.82	3
Kirschmann	20	" e.	19.35	20.7	38.15	1	1.069	1.971	.79	1.47	2.48
Hurst	10	" e.	19.35	20.85	35.1	1	1.077	1.814	1.12	.71	1.4
McKay	20	" c.	21.75	22.075	42.175	1	1.014	1.939	1.15	.81	2.82
Kirschmann	20	" f.	16.9	17.2	29.7	1	1.0177	1.7574	.54	1.31	1.99
McKay	20	" b.	23.25	24.75	40.3	1	1.0645	1.733	1.5	1.15	2.52
Kingstone	20	" f.	20.55	21.2	34.1	1	1.0316	1.693	.61	1.8	3.15
McKay	20	" f.	24.25	25.4	42.55	1	1.0474	1.7503	.975	.95	1.9
"	20	" e.	19.575	20.025	41.95	1	1.0229	2.1425	.9825	.875	2
"	10	" g.	30.8	32.9	50.7	1	1.0681	1.6401	1.76	1.54	2.02
Pringle	20	" h.	17.45	18.22	30.34	1	1.044	1.738	Mr. Pringle's M.V. is not recorded in his results, but in no case does it exceed 2.		
"	20	" f.	25.125	27.875	35.8125	1	1.109+	1.425			
"	20	" h.	15.75	15.55	26.875	1	.987	1.706			

TABLE B.—DACTYL.

Name.	Number of feet measured.	Verse.	Average length of Syllables.			Relative Percentage.			Mean Variation. (M.V.)		
			—	⌢	⌣	—	⌢	⌣			
Alexander	10	I. (a)	29.8	19.4	17.1	1.7426	1.1403	1	2.1	2.36	1.1
McKay	20	" (b)	41.25	20.9	19.2	2.1484	1.0895	1	1.7	.72	.1
Wilson	20	" (c)	43.45	22	23.1	1.8809	.9523	1	2.24	1	2.01
McKay	10	" (d)	39	26.3	24.2	1.6115	1.0867	1	1.09	2	1
Hurst	10	" (e)	29.3	23.5	22.7	1.29	1.035	1	1.54	1	1.14
McKay	10	" (e)	40.15	22.9	20.85	1.954	1.098	1	2.6	.98	.85
McKay	20	" (f)	39.35	24.45	22.4	1.756	1.091	1	2.02	.75	.88
Wrinch	10	" (d)	27.1	23.7	19.55	1.383	1.202	1	.74	.42	.84
McKay	10	" (e)	41.35	22.15	20.1	2.0572	1.1019	1	1.99	.84	1.12
Wrinch	10	" (h)	25.9	21.5	21.45	1.2074	1.0028	1	1.12	1.2	.65
Hurst	20	" (b)	28.9	24.8	22.2	1.3018	1.1171	1	1.1	.84	1.4
Hurst	10	" (e)	31.25	23.3	21.4	1.4579	1.0887	1	1.9	1.26	2
McKay	10	" (e)	36.35	22.95	23	1.5804	.9978	1	2.09	.47	.5
Pringle	20	" (j)	30.5833	19.0416	18.8958	1.819	1.107	1			
"	20	" (k)	22.35	18.3	20.266	1.074	.879	1	M.V. in all cases less than 2.		
"	20	" (j)	27.3	14.6	14.4	1.895	1.013	1			

TABLE C.—IAMBUS.

Name.	Number of feet measured.	Verse.	Average Length of Syllable.		Relative Percentage.		Mean Variation. (M.V.)	
			˘	—	˘	—	˘	—
McKay	10	IV. (a)	20.65	39.3	1	1.903	.78	2.48
Hurst.	10	" (b)	20.05	44.5	1	2.197	1.01	1.15
McKay	10	" (a)	20.5	41.5	1	2.044	.9	1
"	10	" (a)	21.4	43.3	1	2.0233	.92	1.4
"	10	" (a)	21.5	42.	1	1.9116	.7	.6
Pringle	20	" (b)	22.1	31.4	1	1.421	Not given.	Less than 2.

TABLE D.—TROCHEE.

Name.	Number of feet measured.	Verse.	Average Length of Syllable.		Relative Percentage.		Mean Variation. (M.V.)	
			—	˘	—	˘		
McKay	10	III. (a)	42.5	24.85	1.71	1	.8	.98
Hurst	10	" (a)	36.45	25.05	1.455	1	1.51	1.35
"	10	" (a)	37.25	26.25	1.4181	1	1.2	2.55
Pringle	10	" (b)	33	24	1.375	1	Mean variation less than 2.	
"	10	" (b)	31	21	1.4761	1		
"	20	" (c)	30	21.95	1.366	1		

TABLE E.
Pure Metre: Dactyl.

Name.	Number of feet measured.	Average Length of Syllable.			Relative Percentage.			Mean Variation. (M.V.)		
		—	⌒	⌣	—	⌒	⌣			
McKay	10	30.7	20.35	19.05	1.6115	1.1207	1	2.7	1.18	.75
Hurst	10	28.3	24	20.1	1.4079	1.194	1	1.96	1.8	1.12
"	10	26.5	23.85	19.75	1.3417	1.2075	1	1.4	1.18	.6
"	10	26.8	24.8	20	1.34	1.24	1	.45	1.24	.9
"	10	33.2	23.7	21.8	1.523	1.087	1	1.36	.82	.68
McKay	10	33.25	20.95	19.6	1.696	1.068	1	1.8	.58	.52
Wrinch	10	33.8	30.5	17.5	1.9314	1.748	1	1.42	1.9	18

TABLE F.
Pure Metre: Anapaest.

Name.	Number of feet measured.	Average Length of Syllable.			Relative Percentage.			Mean Variation. (M.V.)		
		⌣	⌒	—	⌣	⌒	—			
Hurst	10	18.45	20.65	38.6	1	1.1192	2.0927	.87	1.08	1.74
"	10	17.6	18.85	36.8	1	1.071	2.0914	.7	.78	.7
"	10	17	18.65	32.9	1	1.097	1.935	1	1.15	2.34
"	10	20.6	21.65	38.45	1	1.104	1.984	.48	1.18	1.54
"	10	18.8	20.7	39.75	1	1.101	2.1143	.52	1.36	1.9
McKay	10	19.2	20.6	39.45	1	1.075	2.054	.48	.85	.85
"	10	19.2	19.65	34.4	1	1.023	1.791	1	1.42	.86
"	10	18.6	19.35	32.55	1	1.04	1.75	1.2	1.02	1.54

CONCEPTIONS AND LAWS IN AESTHETIC

BY

A. KIRSCHMANN, PH.D.

CONCEPTIONS AND LAWS IN ÆSTHETIC

I

I once lived in a house where they had a dog, a cat and a parrot. The three ate from the same plate, but the parrot had a certain superiority because he could speak, and indeed speak well. One evening, when the parrot had already finished his meal and was swinging on his trapeze, the cat and the dog quarrelled over the milk, which still half filled the wooden dish from which they ate. "Tom," resorting to an active attack, jumped with his fore feet upon the edge of the wooden vessel, which, having a round bottom, upset and threw the milk all over him, whereupon he quickly withdrew much humiliated. The parrot, immediately aware of the comical incident, set up a hearty and prolonged laugh, just like a man's, and did not cease till everybody in the house had joined in.

I must beg the reader's pardon for beginning a scientific article with an anecdote, which indeed I should scarcely have kept in mind, had the occurrence not been succeeded by a rather animated debate among those who joined in the parrot's laughter, as to *whether the parrot had really laughed.* Most of the company claimed that the parrot, being only a brute, could not laugh, *i.e.*, really enjoy the funny side of the occurrence. His endeavour to imitate the sounds of human beings led him in this case also to repeat the noise which he had heard on similar occasions. But if the explanation is correct, why did the parrot imitate this peculiar noise just when something laughable had happened? This parrot laughed but seldom, and had never been known to laugh so heartily as on the occasion in question. What criterion did the parrot use for associating a certain physical action, laughter, with certain events, if not the funny side of them? Moreover, is there anything more than a difference of degree between a parrot and a child, or even a grown-up person, in this respect? Does the parrot in calling "Polly wants a cracker" act in any way differently from the child, who says, "Mamma, Jimmie wants a candy?" Both, on account of certain experiences, repeat certain words which they have heard and which they believe effective in attaining a certain end. It is plain imitation in both cases. We have no right to regard the intellectual powers which precede and accompany the parrot's speaking and laughing as something essentially different from the intelligence of man when performing the same actions.

At the base of all ordinary intellectual activity of man lies imitation, imitation of one's self (habit) and imitation of others (good example, influence, etc.) All our conclusions by induction,[1] our inferences according to analogy and utility, do not spring merely from our desire for truth, but have some other motive besides. Since it is obviously the same with the animals, we are thus far only different in degree from them. In so far as our thoughts and actions spring from habit and imitation and from the application of principles which do not carry absolute certainty with them, we are nothing but animals, perhaps the highest. Only when we do our actions and decide our problems with no desire for pleasure or advantage other than that which is inherent in truth itself, only when we, without wavering and flinching, follow that divine "word" of truth in us, that spark of godlikeness with which we are endowed, are we something essentially different from other animals. It must be understood, that I do not wish to say that seeking pleasure, acting according to the principle of utility and arguing by analogy and induction are matters low and sinful; but I emphatically hold that stating inferences drawn by analogy or induction, as if they carried absolute certainty with them, or acting according to the principle of utility with the pretence of aiming purely at truth is something *very low* and that it is extremely *sinful* to *seek the agreeable at the expense of truth.*

But the reader will ask, Who does such wicked deeds and who thinks so untruthfully? And we must answer, We all do so, we have become accustomed thereto through a training of thousands of years. We do so not only in daily life, in superficial conversation, but even in our scientific research. We state as certain, as absolutely true, not always what we have experienced, or what we can prove, but very often simply what we wish to be true. Sometimes even when we are solving a mathematical problem, at some point we represent to ourselves or to others a proposition as certain (although we do not know it as certain, but we wish it to be so at that moment), which is not certain or is even false; and then we say afterwards that we made "a mistake." At the base of all errors is a lie. If we never stated anything as absolutely certain which is not certain there could never arise an error, either in common life, or in science, philosophy or religion.

[1] **Of course incomplete induction, for complete induction is deduction.**

Nature (*i.e.*, the immediately given events in our consciousness, perceptions, emotions, etc.) never deceives us; it is always our wilful interpretation which leads us astray. If the traveller in the desert sees a mirage and, approaching, is disappointed when it vanishes, who deceives him? Not nature, not his senses, for they did not tell him more than that certain parts of his field of vision were occupied by sensations of such and such a colour, shape, intensity, etc. But he infers: On former occasions when I had similar perceptions, I found, when approaching, an oasis with refreshing shade and water; since it has been so ten times, or fifty or a hundred times, it *must* be so always, and therefore this time also. But to such a conclusion he has not the slightest right. He only is entitled to say: It may be so, I believe it is so, but it may be otherwise. All our judgments by analogy and our conclusions by induction are of this nature. Thus, all our statements of causal connections partake of this untruthfulness. We attribute absolute necessity to causation, although we gain our alleged knowledge about causal connection by induction. In so far as such connection can be reduced to geometrical laws, they possess absolute certainty. Everything beyond is based on belief.

But not only in our completed propositions, in our statements in ordinary life and often in scientific procedure do we let the wish be the father of what we coin as "thought," but we are guilty of the same untruthfulness already in the formation of the conceptions and distinctions which underlie these propositions. We use a term and make our hearers or readers believe that a sharply outlined, well defined conception corresponds to it in our mind, and we make distinctions with the silent pretension that they are founded in the very nature of things. But in nine out of ten cases we have never accurately defined the term or thoroughly examined the distinction as to its legitimacy. We have simply repeated them as we heard them from others. *We have just done what the parrot does.* Perhaps we have done worse; for the parrot in his utterances is always very concrete and we can easily guess what he wants; he expresses his desire without pretending that his utterances are "knowledge, certainty," as we do.

Whenever we use words without having a clear conception of their meaning, or distinctions whose claim for legitimacy we have not thoroughly examined, we deceive ourselves and others. A great number of our most cherished ethical and religious ideas belong to this category. People have become accustomed to certain words and distinctions; they have adopted them without ever examining them as to their inner truth. They love them, and everybody is a heretic who does not do the same. Do these people ever think what will be left of such "ideas" when all words are taken away? How much of our religious ideas and convictions shall we be able to present to the Divine Judge, if he does not speak to us in any earthly language, English, or French, or German?

When does a word really stand for a conception? It does so when, in the mind of him who uses it, it represents a simple fact or relation, *i.e.*, an ultimate element of consciousness, or when he can define its meaning in terms of such simple elements. There are two kinds of ultimate facts, *i.e.*, of absolutely simple elements of consciousness. When we analyze our complicated experiences we arrive at last at elements which could not possibly be analyzed further, as for instance the quality *red*. These elementary facts have absolute but only assertive certainty; they are so, but they might be otherwise. If we analyze the relation in which these elementary qualities (sense-qualities, emotional elements, elements of memory images, etc.) stand, we arrive at last at elementary relations of an axiomatic character. They are the axioms of geometry. They possess absolute, apodeictic certainty, *i.e.*, they are so and could never and nowhere be otherwise. *Nothing is absolutely certain unless it consists of such assertive and apodeictic elementary facts or can be derived from them by means of the application of the apodeictic facts.* It is therefore the first duty of every science to analyze its material till it reaches the ultimate elements. There are only two branches of human knowledge which have fulfilled this condition to some degree, mathematics and psychology. In mathematics every proposition is directly or indirectly derived from or reducible to elementary axioms, whose apodeictic certainty cannot possibly be denied. Modern psychology has in its analysis of the sense-perceptions in many cases really reached the very elements, and in spite of the spreading tendency to explain every thing psychical physiologically (*i.e.*, to explain the simple and directly given by the complicated and inferred), there have been some attempts really to start with ultimate facts. None of the other sciences in our days have advanced so far with their analysis. They start from assertions, which have not the criterion of absolute certainty. That nevertheless some of them have succeeded in building up universally acknowledged systems, *e.g.*, physics or astronomy, is due to their conscientious application of the apodeictic facts, *i.e.*, the mathematical laws. They form their propositions in such a manner as to require only slight modifications, even if the first presuppositions from which they started should prove to be partially or totally wrong. Further, they push back all uncertainties and contradictions into a few conceptions such as matter, atom, uniformity of nature, etc., which are kept in the background of discussion. The law of gravity is, so to speak, independent of any special atomic or dynamic theory, and the wave-theory of light will be the more correct, the more it emancipates itself from all special theories of matter and ether.

It must be noticed here that the elements arrived at by psychological and mathematical analysis are final, whilst the elements of any other science are so only when they prove at last identical with the psychical elements. The progressive period of chemistry commenced from the moment when it condescended to a true and thorough analysis. But it has not yet arrived at final elements. The chemical constitution of a substance is *known* only when it can be expressed in terms of chemical elements. But the seventy elements of modern chemistry are by no means necessarily ultimate. With a deeper insight they might be proved to be modifications of a smaller number of elements; and such coming knowledge, of which there are already indications, might revolutionize the whole of chemistry again. On the other hand the elements of the psychological and mathematical[1] analysis, (*e.g.* the quality *red*, or the axiomatic intuition that the straight line is the shortest distance between two points), no matter how complicated their designation or expression may be, are absolutely and ultimately simple. They cannot be defined and do not require definition.

Now we claim that any term (alleged conception), which does not designate an ultimate element, and which cannot be defined in terms of such ultimate elements, is a pseudo-conception, a mere word. As such it may be either superfluous, *i.e.*, containing nothing which is not already expressed in other terms, or false, *i.e.*, implying contradictions. All distinctions based on such conceptions are unfounded and illegitimate, and any philosophic or scientific system, in which such

[1] Mathematics could be regarded as that branch of psychology which deals with the apodeictic facts, for all facts are psychical. Nothing can be a fact unless it takes some place in consciousness. In this sense all the special sciences are subdivisions of psychology. It is the task of modern exact and experimental psychology to bring about a complete reform of all branches of human knowledge. When all sciences have arrived at that stage where they express their propositions in terms of real facts, there will be no further need of a special science psychology

pseudo-conceptions and psuedo-distinctions are employed is not knowedge but sham-knowledge.

But what has all this to do with æsthetic? Simply this, that there is no branch of human study which is so far behind in the search for ultimate facts, and there is scarcely another territory of profane or sacred, everyday or scientific discussion in which there is such a superabundance of pseudo-conceptions and pseudo-distinctions as in æsthetic. Scarcely in any field of intellectual activity are we so much like the parrot as in our discussion of the products of fine art.

The science of æsthetic is still in a kind of mediæval stage. Whilst physics formulates its propositions and laws irrespective of any assumptions about the nature and even the existence of matter, whilst psychology has long ceased to start with the conception of the soul, æsthetic still takes as the foundation on which all its considerations are based the "idea of the beautiful." Instead of starting with undeniable facts, or at least with something which may pass as experience, it starts with a preconceived idea. The "beautiful" is something absolutely different from the agreeable, the æstheticians say, but they never prove it. It is simply so, because they wish it to be so, for they imagine that the agreeable (pleasure) is something base, something low and sinful. On account of this absolutely unfounded pseudo-cultural and pseudo-christian assumption (for the agreeable is low and sinful only when sought or obtained at the expense of truth), they then make frantic efforts to define beauty as something absolutely independent of pleasure. These attempts result in either proclaiming as criterion of beauty what can be attributed to everything, or in the introduction of terms which silently imply pleasure, thus begging the question. To the former kind belong the definitions of beauty as the "unity in variety," the "unity of idea and phenomenon," "infinity in the finite," etc. To the latter kind belong those views which reduce beauty to "proportion" or "harmony," for these terms have only a distinguishing significance when applicable not to all, but only to certain relations of numbers or other magnitudes (*e.g.*, Hogarth's curves or Zeising's golden section). The most vicious of such circles is exemplified by those theories which "explain" the æsthetic effect by "association" — the philosopher's stone of empirical psychology. The selection of what is to be called proportionate or harmonious implies of course the reference to the agreeable. Even Kant's "interesseloses Wohlgefallen" (pleasure without interest) belongs to this category; it is a term which contains its contradiction in itself. The criterion of pleasantness is interest, and therefore pleasantness without interest is a notion like a "body without extension."

"Agreeable" and "beautiful" either cannot be both simple (ultimately elementary) qualities, or they cannot overlap. Thus the beautiful is either a special case of the agreeable or pleasant, or the two qualities must be exclusive of each other, *i.e.*, beauty must be absolutely indifferent. Indeed from Plotinus to Kant and Schiller and later times, it has been claimed that "beauty excludes desire." But this is in contradiction with the directly experienced facts. There may be the absence of some desires but not of all. If we stand before the Venus of Medici or one of Murillo's Madonnas, the desire to embrace her may be absent, but there is certainly the desire to have the beautiful impression continued, for we admire the beautiful forms and features for a quarter of an hour at a time, and often enough even after taking our leave of her we have to turn back and see her again. And if beauty is incompatible with desire, why then do the artists strive to produce the beautiful and why does the public seek after such works? Kant's sharp demarcation of the æsthetic consciousness from the intellectual on the one side and the sensuous and moral on the other is wholly dependent on his strict adherence to the Wolffian psychology, with its faculties of the soul, and to certain common-place distinctions, which in the spirit of his criticism should not have had any place in his system, such as theoretical and practical, real and apparent, form and matter, etc.

Lotze distinguishes the pleasant and the beautiful by relating the former to passivity (suffering), and the latter to activity. But this is simply to beg the question; for the terms active and passive are meaningless, unless they are based on the distinction of the pleasant and unpleasant. In the inorganic world there is no such thing as activity or passivity; there are only different forms of motion. In the organic world, whether a movement is passive or active depends entirely on the accompanying pleasant or unpleasant feelings. But worse than all these definitions is the tendency of modern authors to "reduce" beauty to "expression," "significance," the "excellent or characteristic," and other vague terms which themselves are ten times more in need of definition than "beauty."

Besides the definition of the nature of the beautiful and its demarcation from the ugly, the sublime, the comic, etc., there are other pet ideas which are treated by certain authors with extreme and altogether undue devotion, *e.g.*, that of the universality of the laws of beauty. As they did not like the idea that the beautiful is a special case of the agreeable, so they do not relish the fact that æsthetic judgments are subjective, for if they were, all the many volumes filled with a philosophy that begins at the second story and does not trouble about the foundation, all the beautiful talk about beauty would be worthless trash. Therefore the laws of æsthetic are objective and universal, in spite of Kant, and in spite of every day's experience which testifies to the contrary. One would think it could scarcely be denied that the recognition and appreciation of beauty depend on race and occupation, on sex and age, and a hundred other circumstances. Scarcely ever (and fortunately so) do different individuals agree as to human and other beauty. The ideas of human, architectonic and ornamental beauty differ widely in a European, a Chinese, a Hindoo, and a Negro, even if they have all reached the highest standard of education. It is the fashion to be orthodox and to proclaim the ancient Greek sculptures as the *non plus ultra* of the representation of human beauty. But apart from the fact that the admiration of the truly unsurpassed observation and skill shown by the ancient masters of plastic art and the enchantment exercised by the beauty of their subjects are two different things, most of us northern people of Teutonic and Celtic descent find the features and forms of women of our own or a kindred race more beautiful and graceful than the Venus of Milo. Many even who give the prize to the latter would withdraw their patronage if they were not allowed to endow, in their imagination, the Venus of Milo or the Medicean with a rosy complexion and fair hair.

How widely do the views of the æstheticians and art critics differ even on the most vital points! It is of comparatively little significance that the authorities cannot agree as to whether the Dresden or the Darmstadt Madonna is the original work of Holbein, or whether Laokoon is crying out or not. But if one distinguished writer on æsthetic makes Shakespeare a classical author, marking the culmination of a long period, whilst another counts him among the founders of Romanticism, one cannot help thinking that there must be something totally wrong in that distinction of classic and romantic. Shakespeare himself certainly did not know whether he was a classicist or romanticist, and if this ignorance did not prevent him from accomplishing unsurpassed and immortal[1] works, why should the reader burden himself with such pseudo-knowledge? And if one author (Winckelmann) holds that the plastic art of the Hellenic nation seeks only to express heroic greatness, composure, calmness of great souls, and another (Hirst) asserts that Greek art does not shrink from the expression of even the greatest horror, and if both as a demonstration of the correctness of their views refer to the same sculptures, there must be something wrong with these "objective and universal" laws of æsthetic judgment. How is it possible that, while the majority of artists and critics look at Hellenic art as at the most exalted and abstract ideal, some distinguished authors (Bosanquet), see nothing in it but clever imitation of nature, void of deeper expressiveness, at best attempting allegory but never rising to true symbolism? Now, such a diversity proves either that the laws of æsthetic judgment are not universal or that there is something illogical in the alleged knowledge of the above propositions. The sharp distinction between allegory and symbolism, it seems to me, is forced and illegitimate, but that may be on account of my ignorance, which I willingly admit; indeed I must confess, that there are common and more familiar terms whose meaning and significance I never can see. Thus I do not understand what is meant by "classical," unless this term is simply used as denoting a period of the history of art, or as a kind of general superlative. As a designation of a specific characteristic of works of art or of the artist, it is for me nothing but an empty word; and I believe that there are many people, even of literary and scientific attainment, who are just as ignorant as I am in this matter, but who accept and even make use of this term simply on the authority of others and without ever asking themselves whether the expression stands really for a well defined and sufficiently outlined idea. Many a reader will smile at this confession, but I must insist that only he who is able to define "classical" in terms which themselves do not require definition, *i.e.*, in terms of simple assertive and apodeictic facts, shall throw the first stone at me. If it is possible to lessen the bluntness of this confession of ignorance by a more blunt one, I should say, as a psychologist, that I have never been able to see the legitimacy for instance of the distinction between con-

[1] That is, as immortal as possible in the age of paper. It is very fortunate that the ancient Chaldeans and Assyrians did not have paper, rubber stamps and sealing wax but wrote on clay tablets, otherwise we should not know very much of them, and we should still regard the early Hellenic period as the dawn of human culture.

sciousness and self-consciousness, unless one of these terms is arbitrarily defined.

The above diversity of opinion about the character of Greek art is due to a fusion of several errors. First: Even if imitation of nature and idealism were the antagonists which it is alleged they are, they should be applied independently to the choice of the subject and to the manner in which the artist represents it. The same artist might be an extreme idealist with regard to the choice of his subjects, and a radical naturalist in the execution, and *vice versa.* Second: Imitation of nature and idealism are by no means antagonistic and exclusive of each other. Every artist, if he aims at something else than what the photographic camera and the phonograph would accomplish, is guided by some ideal, which either as a whole or in part exists or at least could exist in nature. In fact there is in spite of all idealism nothing unnatural in Rafael's Sistina or in Murillo's Conceptio Immaculata, and the beautiful heavenly features would lose nothing of their ideality if it were found out that they were real portraits. Many of the most exalted scenes in Shakespeare's or Schiller's plays may be and have been exceeded by real occurrences; for life is often stranger than fiction. On the other hand no artist can avoid imitating nature if he wishes to be true; no matter how high his ideals soar, the elements out of which he builds up his composition must be true, *i.e.*, possible in experience. Thus we should not speak of idealism in contradistinction to imitation of nature, but we should classify the works of art, with regard to choice of subjects, according to their ideality, the degree of which ranges from zero to infinity; and with regard to the execution, the manner in which these ideals are represented, we should speak of true and untrue representation.

If the laws of æsthetics were really universal, there should be as little dispute about them as about those of mathematics. It should not have been possible for that quarrel to arise about the three unities (time, place and action) on the stage. There should have been no opportunity for a controversy like that in Lessing's time about the limits of poetic and formative art. Or, how can one critic call the precursor of modern plain-air painters, Velasquez, a mere master of technique, whilst another critic sees in him the most objective of all painters, who never shows off, never uses tricks? How is it possible for one to speak of him as a sordid soul who never saw beauty, and another praise him as the artist who saw through the mystery of light as God has made it? How could there be, if the æsthetic laws were objective, any further doubt whether the sharp outlines of the pre-Raphaelites deserve greater credit than the much disputed and much worshipped modern mannerism of treating outlines and perspectives as if they were painted with short-sighted eyes and for short-sighted people?

So far even goes the uncertainty of the æsthetic laws that the æstheticians themselves are by no means unanimous in their decision as to which human activities belong to the fine arts. Some even include among them rhetoric and oratory, which I would prefer to rank with the arts of tricksters like palmists, professional hypnotists, phrenologists and fortune-tellers, and far behind those of the ventriloquist, sleight-of-hand man, and tight-rope walker. For of these three the latter practises his art with entire honesty, and the former two do not coin as truth the illusions which they produce; but the orator wilfully deceives, whenever he exercises his skill, *i.e.*, when he tries to persuade or carry away his hearers by something else than the truth of his argument.

In the limited space of these introductory remarks I can only touch upon a few of the numerous vague and contradictory conceptions and distinctions, the unverified acceptance of which forms, so to speak, the key to the "understanding" of the chaos of æsthetic theories. On reading modern books on æsthetic or criticisms of works of fine art one can hardly suppress the observation that the "play impulse" of Schiller and Spencer seems to exercise its creative force not only in fine art itself but also in æsthetic and art criticism, resulting here in an elaborate and intricate play with high-sounding but undefined words and arbitrary, unfounded distinctions. But this play in æsthetic, history of art and art criticism differs from the play in real art by producing its "semblance" not "without deception."

There have been since Kant and Herbart many earnest attempts at a more exact æsthetic, characterized by greater consistency and less superficial verbosity, but their efficiency is greatly frustrated by the unwillingness to condescend to a thorough analysis down to the elements. The few bricks we possess of warranted solidity to be utilized in building up a trustworthy edifice of æsthetic science are not to be found in the voluminous works on æsthetic and history of art, but in the works of modern psychologists on the psychology of light and colour, spatial forms and musical tones (Fechner, Wundt, Stumpf, etc.). Bosanquet says of the stage which æsthetic had reached at the time of Kant: "A positive or concrete structure of æsthetics is as yet indeed only in the making. The outlines are firmly traced and the materials are lying around in heaps, but the building is hardly begun."[1] So favourable a view seems not to be justified even at the present stage of æsthetic science. Instead of outlines traced firmly with mathematical definiteness we find a chaotic mass of disputable opposing theories, and what is "lying in heaps" is only in part "material," and the greater part is rubbish and dust, which will fly when once a genuine storm of true criticism blows.

I declare again: *All expressions used in æsthetic and art-criticism which can not, unambiguously and without contradiction, be defined in terms of really simple elements (assertively or apodeictically true and not requiring proof and definition) are nothing but pseudo-conceptions; and all distinctions and classifications into which such expressions enter are illegitimate or pseudo-distinctions; and all alleged knowledge based on such conceptions and distinctions is sham-knowledge; and if the originators and propagators adhere to such expressions after they have realized the truth of what is said above, it is not only sham-knowledge, it is then imposition, deception, fraud.*

II

I may proceed now to illustrate the foregoing general discussion by reference to some particular subject in art. I shall first discuss, in the light of what has been said above, the criticism of the art of painting, and afterwards turn in particular to the questions of light intensity and colour-combination.

A picture is a surface (a part of our field of vision) consisting of smaller surfaces which differ in space-relation (extension, shape, arrangement), light-quality (colour-tone and saturation), and light-intensity. All properties which the picture as a whole or in its parts possesses, must be reducible to qualities or relations of these small surfaces. Consequently any quality attributed to the work of art or its parts must be capable of being expressed in terms of these five or six variables. Any alleged physical or æsthetical characteristic of the picture, which cannot thus be unambiguously defined in terms of these elements is not a property of the picture at all. We must be able to state what combination of space-configuration, colour-tone, saturation and intensity we mean, when we speak of "composition," "idea,"

[1] Bosanquet, *History of Æsthetic*, page 283.

"*colorit*," "values," "technique," etc., or else these expressions are meaningless, although high-sounding words, with which we deceive ourselves partly and the hearer totally. Some of these terms are satisfactorily defined; some others could be defined if those who use them would take the trouble. But the great majority of them have never been properly defined; at a sincere attempt to express their meaning in terms of the above ultimate elements they would reveal themselves as superfluous or false, as pseudo-conceptions, as mere words. Those who introduced them had not a clear idea of what they meant by them, and those who used them afterwards, uncritically repeated them. The objection might be raised that most of the terms in question *are* defined, that almost every author takes care to state what he means by his expressions, except in the case of terms which are understood by everybody. But just these common terms, whose meaning everybody claims to know, are often the worst pseudo-conceptions. It is true, most writers define their notions, but they do not define them in terms of the simple variables. They often run round in a circle with their definitions, or at least act like one who would measure distances by triangulation but without measuring a base.

There is the distinction of realism and idealism which is often applied to paintings and painters. Now this contradistinction is either perfectly arbitrary, and then it has nothing whatever to do with the meaning of the terms "real" and "ideal," which are neither in contrary nor in contradictory opposition, or else the terms do not exclude each other. But the æstheticians insist that the distinction is not an arbitrary one but that it has its foundation in the very nature of things, and that the two terms exclude each other completely. They never investigate, however, whether or not idealistic and realistic are ultimate qualities or reducible to such. If they had done so, they would have found that the term realism is the most vicious pseudo-conception that philosophy and æsthetic have ever created. To an unprejudiced mind it is evident, that there is more right to call Rafael's Sistina realistic (if this term has something to do with "real" or "fact") than to apply the term to the products of those modern painters who find reality real only where it looks dirty. Moreover, how is it that no artist by himself knows whether he is an idealist or a realist? He usually has to learn it from his critics; and they sometimes only find it out long after his death.

I can quite well understand what is meant by the contraposition, in philosophy, of idealism and materialism; or in æsthetic, of idealism and cynicism, but, with the utmost effort, I cannot see what could be meant by opposing realism, naturalism or impressionism to idealism. The most ideal landscapes I have seen were not painted but real ones, Alpine and Arctic scenery, for instance, and the features of Rafael's or Murillo's Madonnas are perfectly possible and may even have been surpassed in nature. The difficulty with the terms "real" and "reality" is a double one. First, it is forgotten that if there were no wilful deception in the world, if nobody were to transgress the law of truth, there would be no place at all for these terms, for there would be nothing unreal. Second, the terms "reality" and "real" are used ambiguously. Sometimes they are employed in contradistinction to the products of human deception, and sometimes they are applied to a part of experience which is characterized by certain relations in visual and tactual space. Everything which has not these relations is then called "unreal." Thus, for instance, the perception of an object is "real," but the memory image of that object, because it lacks localization in the vision field, is called "unreal." This is perfectly arbitrary. The memory image is something actually existing. It is a fact, and that it lacks certain properties which some other facts possess does not give us the right to treat it as if it lacked all properties of actuality, as if it were a product of deception. The confusion of these two meanings of the unreal, which is now identified with deception, and now with that which is not palpable in space, is the most vicious source of errors in common life, science and religion.

Some critics have a certain preference for the word "technique," whose whole force lies in its ambiguity, for it means sometimes a property of the picture, and sometimes a quality or a method of the artist. If technique is a property of the artist, we must not blame or praise the picture for it; but if it is a property of the work of art then we must treat it independently from the action of the artist who produced it. But here is just the basis of the fallacy. Those who use the term technique to designate certain characteristics of the painting or its parts seem to think that the same property of the picture must always be produced by the same manner of painting, which is absolutely wrong. The same effect on the canvas may be accomplished by a hundred different ways of painting, and some of the most distinguished painters have accomplished masterpieces by what the average connoisseur would regard as altogether heretical and dilettante-like methods.

As an example of a term which would very well be satisfactorily defined in terms of ultimate elements, but which is mostly surrounded by a veil of mystery, impenetrable for the uninitiated—and the initiated seem to like such veils—we might mention the *chiaro-oscuro* (*clair-obscure Helldunkel*). The consciously chosen contradiction in the name already shows the confusion of thought in the mind of the originator. One speaks of the *chiaro-oscuro* as if the painter could accomplish the impossible, make his canvas transparent, magically produce bodies on a flat surface and paint a veritable translucent atmosphere, or even empty space. But all that is at the painter's disposal is the juxtaposition of more or less extended surfaces of different intensity and quality. Even some noted scientific writers seem to think that we could see one intensity through another, or one colour through another. They say: Even in the deepest shadows on a snow-field the whiteness of the snow is perceptible, and if we hold a yellow glass before the eye and a blue object at some distance, we can distinctly see a blue surface through a yellow medium. But in making such statements they express not what they really perceive, but what they suppose it to be. If the necessary precaution is taken to exclude all knowledge about the situation, so that the observer has to rely for his statements on what he really *sees*, *e.g.*, if he looks through a tube which conceals the edges of the surfaces in question and which is long enough to prevent him from noticing little inaccuracies on the surface of the glass or the grain of the paper, he will not see two colours, one through the other, but only one. Our sense of sight has not the faculty of seeing at the same time different qualities at the same point of the vision-field. It responds to a manifold stimulation with a sensation of one simple quality and one intensity. The presentation of transparence, in so far as it is perception (*i.e*, in so far as it is not obtained by inference through analogy, etc.) is produced by parallactic relations between the images of the two eyes or between the successive images of the moving eye. The painter is utterly unable to produce such parallactic relations on the canvas or paper. That by painting transparent pigments over each other or on top of opaque ones he very often succeeds better in obtaining the desired colour-tone and intensity, than by applying directly an opaque or a mixed colour, has absolutely nothing to do with any alleged perceivable transparency of parts of the picture. The same effect may be produced, as it is indeed done with gouache or pastel-colours, with absolutely opaque pigments. Pigments may be transparent, perceived coloured surfaces are never so, and all the beautiful phraseology about painting "air," a "transparent sea," an "endless or hazy perspective" is nothing but inaccurate talk by people who have not sufficiently analyzed the problems which they discuss. However, this superficiality of the æsthetic critic has so far misled the public only and not the artist. For the experienced artist knows quite well that he has to produce everything by the juxtaposition of differently coloured dots on the surface. He knows quite well, even if he is not able to express it in conventional scientific terms, that white is not a simple quality like the colours, but that we need some other data than the mere intensity and quality of light in order to call a surface white.[1] He knows perfectly well that identically the same mixture of pigments may at one place stand for white, at another for black, and that he can paint without a single white stroke what everybody will immediately recognize as snow. If the reader is not perfectly convinced of the foregoing, the following simple experiment may be recommended. Let him take a piece of black and another of white paper. Let him fasten them to the wall, but place a screen between them, that they may be illuminated separately. Now it is very easy to illuminate them in such a way that the black paper looks brighter than the white (for instance by exposing the black to the sunshine or an arc light, whilst the white is kept in the shade). If a pair of card-board tubes, darkened inside, be then placed in such a position that through one of them a part of the dimly illuminated white surface is seen, and through the other a part of the strongly reflecting black, and nothing else, especially not the edges of the papers or the screen, then an observer, when asked what he sees, will only be able to state that he sees two colourless surfaces of different brightness. And if requested to use the terms black and white, he will call the brighter, *i.e.*, the black surface *white* and the darker, *i.e.*, the white *black*. Although these facts are well known, the proposition that black and white are not elementary sense-qualities is far from being generally accepted, not because any thorough-going argument could be advanced against it, but simply because it is not liked for some reason or other which has nothing to do with science and truth. It is here as with many other commonplace and scientific prejudices. We persist in saying that in lamplight all yellow objects

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[1] See the author's article on colour-saturation and its measurement. (American Journal of Psychology, vol. iv., p. 542, *et seq.*)

look white, whilst a simple experiment which everybody may perform clearly proves that we should correctly state that in artificial light (lamps, candles, incandescent electric light, etc.) all white objects look yellow (orange-yellow). We persist in regarding the eye as "the mirror of the soul," although we know quite well that the eye is not capable of any other expression than that which depends on the curvature and moisture of its visible surface, and on the direction of its axis, and although we know quite well that we cannot see any expression at all, as soon as we cover, by a mask, everything else of the human face but the eyes.

But there is another point in the problem of the *chiaro-oscuro*, where a more profound scientific analysis would have been extremely beneficial to the progress of the art of painting, namely the inquiry into the limits of intensities producible by the painter. A practical problem may lead up to the theoretical consideration. Cover a surface half with the best black and the other half with the best white pigment obtainable, and then let a shadow fall or put a strip of black velvet across a part of the black. Now ask a painter to reproduce it on the canvas or paper. Will he be able to do this? His best white pigment will be just bright enough to represent the white part of the surface, and his best black will just be dark enough for the black part; but how will he ever paint the shadow or the velvet? If he used his best black for this purpose he would have to represent the unshaded part of the black surface by a gray only, thus abandoning correctness. It will be utterly impossible for the painter to reproduce these three surfaces in their correct intensity differences. But may he not reproduce them in their right proportion? Mr. Ruskin, who touches on this point in his *Modern Painters*, seems to think so; but it is a mistake. If we could reproduce the right proportion, that would be all that is required, for our sense of sight does not care about absolute differences, in fact it is not able to recognize them. It is just the great law of psychical relativity, that we judge always according to ratios, not differences. But the intensity relations which we find in nature are not confined within the same limits as those which are at the disposal of the painter.

The reflection-coefficients of the best black and white pigments have been found to be to each other at best as 1:66, that is to say, the brightest spot of any painting is never more than 66 times brighter than the darkest. If we make an allowance for the relief effect of colours laid on very thickly and with clever regard to the illumination, the above ratio might be raised a little, let us say to 1:75 or 80. Even if the surfaces of the objects were to keep, with regard to their reflection of light, within these same limits, the perceivable intensity ratios would yet not do so; for the surfaces in nature are not arranged, like those of the picture, in one equally illuminated plane; the black may be in the shade, the white in sunshine. There are, further, different sources of illumination; and finally there are the light-giving bodies themselves. That even apparently small intensity differences are out of reach of the painter's brush can be seen from the following examples.[1] By measurement with the polarisation-photometer the ratio of intensities between a black and a white cardboard, the former being in diffuse daylight, the latter in direct sunshine, was found to be approximately 1:600; and if the black cardboard was in the shade, the white in the sunshine, this ratio was raised even to 1:3700. A gas flame was found to be 1600 times brighter than a white cardboard placed at a distance of 1½ yards from the gas flame and illuminated by the latter and by diffuse daylight. The gray sky on an extremely dreary rainy day was still more than 420 times brighter than a white painted crossbar of a window seen against the sky as background. Very bright white clouds, illuminated by the moon, were to the surface of the latter herself in intensity as 1:340; and the intensity-ratio between the moon and the clear sky was found to be 4800:1. The artist is utterly unable to reproduce these relations. If he wanted to paint the gas flame in the above case and were to use his best white for it, the deepest black which he is able to apply would still be more than 20 times too bright to represent the *white* cardboard. If the ordinary sky on a rainy day is beyond his means, how should he ever presume to paint fire, the moon and even the sun? It has been said, as was stated above, that he cannot reproduce the actual intensities, but that he may paint them, reduced in their absolute value, but in their right proportion. But this is simply impossible; for the above numbers *are* the proportions. The originator of that idea obviously had it in his mind that with respect to the dark the artist was unlimited, *i.e.*, that his darkest black was absolute absence of light, which is by no means the case. This darkest black, no matter whether it is lamp-black, ivory-black, Paris-black

[1] See the author's article, *Die psychologisch-æsthetische Bedeutung des Licht und Farbencontrastes.* (Philos. Studien, VII, p. 362, *et seq.*)

or obtained by a mixture of brown and blue colours, represents still a very considerable intensity. An ordinary black surface, as for instance black cloth or a painted black (not to speak of india ink or printers' ink, which do not appear black at all when exhibited in large surfaces) looks only dark grey when compared with a shadow thrown on it, or with good black velvet; and on black velvet one can very well see the shadows, which are still blacker. But even these are not as dark as an opening into an almost absolutely dark space.

Thus the means at the disposal of the painter are of such a nature, that tho intensity relations which he is able to produce on the canvas or paper can never reach or exceed the ratio 1:100. That he still succeeds in some cases in effectively representing somewhat greater intensities, he owes to the laws of contrast which he consciously or instinctively applies. According to the laws of light contrast a small bright surface may be considerably brightened by great surrounding dark surfaces, and *vice versa*. By cleverly making use of this law he may even raise the intensities of very small white, yellow or orange surfaces so as to give the appearance of a certain luminosity, as, for instance, in the case of glowing coals and sparks of a smith's fire, the illuminated windows in an evening landscape, the Alpine glow, etc. But he should never try to paint the flames of candles, lamps, or torches themselves, or the celestial bodies, for this is, with respect to true reproduction of intensities, absolutely impossible. Indeed when we see the sun or moon on such a picture, we recognize it not by its intensity but by its shape and surroundings. Man has become accustomed through a thousand years of training to represent nature by projections on the plane often without regard at all to intensities and colour. A drawing which consists of a few horizontal lines and a circle or semi-circle above, he will immediately recognize as a "sunset" or "moonrise," although the most characteristic property of these events in nature, the intensity-difference, is absent, and although the drawing has ten times more similarity to a "penny on the floor" or "a plate in a cupboard." Painters who select such subjects for their works are either poor observers of nature or they are misled by critics who have never realized the problems involved. The great artists, and especially those who are praised by the critics as the masters of the *chiaro-oscuro*, are always very careful to hide the source of the light in their paintings. Thus the great landscape painter Calame in his "Sunrise on Monte Rosa" succeeds very well in representing all the beautiful effects which the rays of the glowing morning sun could produce on the snow-covered peaks, but he did not paint the sun itself. He would have spoiled his picture if he had done so. And Rembrandt in his masterpiece, "Burial of Christ," with its beautiful antagonism between daylight and the illumination by torches, carefully avoided letting the flames of the torches be seen.

We have seen in the foregoing considerations how far the painter is from being able to represent the light-intensities found in nature in their correct relations. What then should we think of that tendency of modern artists, and often of just those who like to call themselves naturalists and impressionists, voluntarily to confine within still narrower limits the small compass of intensity-differences which is at their disposal according to the nature of the pigments. The ratio 1:60 or 80 is still too high for them and they do not go higher than 1 to 20 or 30. It cannot be denied that this modern way of painting in many cases reaches a greater truth in colours, but it is mostly done at the expense of the truth of light intensity. It is the fashion now to find the sombre colouring of the Spanish and Dutch School completely at fault, and to praise the gray haze of modern plain-air painters, as the *non plus ultra* of correct representation of nature. Someone has said: "Rembrandt made his people live in an atmosphere of his own invention." I think this could be asserted with greater truth of modern impressionists. Rembrandt sacrificed the truths of colour tone to a certain extent, in order to come nearer to truth with regard to light intensity. He chose the sombre brown tone, not out of caprice or an inclination for mystic dreaming (Fromentin), but because the yellow and orange side of the colour-manifoldness (brown is always a shade of yellow or orange) admits of the greatest number of intervals between full saturation and the darkest shade. He knew quite well that a general tone which spreads over the whole picture is almost irrelevant if the picture is placed in the right environment. But these moderns sacrifice all truths of intensity for a certain mannerism in colours. They paint everything in a kind of purplish gray haze, and they call that impressionism, although such impressions can only be obtained by seeing things in a veiled mirror or through a cloud of cigarette-smoke.

Finally, as leading up to the subject of the next article, we may glance at the reasons why the science of æsthetic, in spite of so many volumes filled with the theories of the beautiful, has hitherto not succeeded in establishing a single really unquestionable rule about colour-combination. Even more, it has not yet succeeded in establishing a consistent system of designations for colour qualities. The æsthetician makes use of almost the same confused terminology as the drygoods salesman, giving different names not only to different colour-tones, but also to different intensities or degrees of saturation of the same colour, and, on the other hand, the same name to different colour-tones. Moreover, people use these names often without themselves knowing what they mean. Everybody of course "knows" what *crimson, indigo* or *purple* is; but if you ask a number of people to pick out these tones from a set of 20 or 30 colours, representing the colour-circle, the decisions vary enormously. Some highly educated persons will not know whether crimson is only a darker shade of red or an altogether different colour-tone; the terms *purple* and *violet* are a source of continuous confusion. Even the terms for the four colours whose names can be traced back to Aryan roots do not mean the same to different persons or even to the same person at different times. Most people, for instance, are not aware that the green in nature is not a pure and saturated green, but more of a grayish yellow green. They are greatly astonished when they see for the first time a green leaf beside a spectral green. Bricks and hair sometimes are said to be red, but if a book is bound in cloth of identically the same colour, nobody will call it red. The very same colour-tone is named quite differently when seen in different objects. The whole controversy between Gladstone, Geiger, Magnus and Grant Allen and many others about the development of the colour-sense in man during historic time was caused by this inaccurate colour-nomenclature. With the same right as it was maintained that the ancient Greek and Hebrew did not see blue we could assume that the German peasants do not see violet, for they call it blue, and that they cannot distinguish red and brown, for they call the fox red and they speak of red horses and cows.

It is clear there cannot be universally valid rules for colour combinations unless there is first a consistent and unambiguous system of names for colour qualities. It is no sense at all to say, "green and blue do not agree together" if we leave some doubt as to what we mean by "green" and "blue." This same example may also serve to demonstrate another and indeed the chief reason, why we have not yet arrived at something better.

Let us examine the above proposition that green and blue do not agree together. Everybody certainly can distinguish at least twenty different greens, ranging from yellow-green to blue-green. In the same way there are at least twenty different colour-tones which have a claim to the designation blue, from the greenish to the violetish blue. Between these 40 different colour-tones there are 20×20=400 different combinations possible. So far we have dealt with fully saturated colours; but it is certainly not exaggerating to assume that we could at least distinguish 50 different degrees of saturation for each of the above 40 colour-tones. That would bring the number of possible combinations up to 400×50×50=1,000,000. Finally we must admit that each of the components is variable in light-intensity. If we assume that we could distinguish also 50 different degrees of brightness in each component, our number of possible combinations of green and blue would assume the value 1,000,000×50×50= 2,500,000,000. But we are not yet at the end of the possibilities. Even if we find among the above 2,500 million not a single pleasant combination, this will be only valid for the case that both components are equal in shape and extension. If we vary these space-relations we might yet arrive at combinations of a pleasant character. This variability in space-relations makes the number of possible combinations practically unlimited. Even the absolute size is not irrelevant, a circumstance which is very often neglected in architecture and decorative art. An ornament or the combination of colours chosen for a building, which, when executed on a small scale, *e.g.*, on a plan or model, makes a very agreeable impression, may, when carried out in the building itself, be entirely unsatisfactory. In the model the colours stood under other contrast influences than in the finished structure, whose surfaces cover greater areas of the vision field.

Thus, until it is proved with mathematical certainty that two colours do not agree together and why they do not, or until the great number of possible combinations is to some extent sufficiently investigated, a statement such as that about the disagreement of blue and green means simply nothing. As long as the above stated conditions are not fulfilled and the contrary proven, we have the right to say that *any two colour-qualities will make a satisfactory combination if selected in the right intensity, saturation and space relations.*

EXPERIMENTS ON THE AESTHETIC OF LIGHT AND COLOUR

FIRST ARTICLE

ON COMBINATIONS OF TWO COLOURS

By

EMMA S. BAKER, B.A.

CONTENTS.

I. History of the Problem. The æsthetic value of single colours 29

II. History of the problem (*Continued*). The æsthetic value of of colours in combination 35

III. Experimental work 42

IV. Anticipatory remarks to further publications............. 72

EXPERIMENTS ON THE ÆSTHETIC OF LIGHT AND COLOUR

I

HISTORY OF THE PROBLEM

THE ÆSTHETIC VALUE OF SINGLE COLOURS

From the very nature of ancient and mediæval thought, we should not be led to expect that a theory of the æsthetic value of single colours would occupy much, if any, of the thought of those times, and indeed the literature of æsthetics down to the time of Kant contains little on the subject. In a historical survey it is sufficient therefore to begin with Kant and to notice after him those modern writers who have given some attention to the subject.

Kant's theory of the beautiful is so bound up with his conception of the form and matter of knowledge that no brief statement could give an intelligible account of it. He places beauty altogether on the formal and therefore universal side of experience, and since all sensations fall under the heading of the matter of perception, his theory could not possibly allow that colour as such could in any way be beautiful. A close reading of passages in which he seems to give sensations an æsthetic value will show that in such cases he is merely conceding something to a popular use of terms, and that what he says has no direct relation to his theory of the beautiful. And even in these concessions, for example where he speaks of the purity of colours constituting their beauty as a "kind of formal determination of them," he says: "This (purity) is besides the sole element in them which is universally communicable, for we cannot assume that the quality of the sensations is for all subjects the same or that preference is adjudged to one colour before another........ in the same way by every one."[1] From this quotation we can easily see that whatever Kant meant by 'purity' he certainly did not use it in any sense which is in common use. Caird adds, in his exposition of Kant, certain references to the physical theory of colour

[1] Caird, *Critical Philosophy of Kant*, Vol. II, p. 427, quoting from Rosenkranz's edition of Kant's works, Vol. IV, p. 71.

as ether vibrations, and concludes as follows: "On this view, pleasant colours or tones furnish merely the matter brought under the form of beauty, which may be part of the charm or attractiveness of objects, but strictly speaking has nothing to do with their beauty." [1]

Herbart, on the other hand, is perfectly definite, and his position is unequivocally that single colours have absolutely no æsthetic value.[2] The reason for this conclusion, however, is not to be found in his colour theory, for such he can hardly be said to have, but in his theory of being, in which he starts always from the simple. Emotions are nothing but the result of combinations of presentations (ideas) and therefore, of course, no simple idea could have æsthetic value.

Among distinctly recent writers, Chevreul and Bosanquet hold views diametrically opposed. Chevreul was one of the first to emphasize the æsthetic value of single colours. A brief quotation will show his standpoint. He says: "Everybody whose eyes are properly organized takes pleasure in looking at a white sheet of paper on which are falling the coloured rays transmitted from a coloured glass, whatever be the colour of the glass." [3] We notice here that for Chevreul not only do single colours have æsthetic value, but all single colours are agreeable. Bosanquet, on the contrary, thinks that colours in isolation have no æsthetic value.[4]

Grant Allen's theory of the colour sense can only be understood by taking strictly evolutionary ground and thinking of life as a whole, and of the various factors which make up life, even down to the simplest sensation or feeling, as occurring under the lead of the principle of the survival of the fittest. Those beings have survived which were best able to compete in the struggle for existence, and those plants which are best able to sustain life have survived and multiplied largely through the relation of animals to them. This is equally true in the realm of the special sensations. Allen contends that the origin of the colour sense and of coloured objects, (flowers, fruits, etc.), is directly connected with this struggle for existence, and the survival of the fittest. Those colours are the most agreeable, which are the colours of objects capable of sustaining life. The development of the agreeableness of individual colours has

[1] *Ibid.* p. 428.

[2] Herbart, *Werke*, Vol. I, p. 173.

[3] Chevreul, *De la Loi du Contraste Simultané des Couleurs*, p. 82 (par. 174).

[4] Bosanquet, *History of Aesthetic*, pp. 385 and 415.

been taking place for ages, until it is to-day merely instinctive. Red, orange, etc., are thought to be the most agreeable colours, because these are the colours of the fruits which played such an important rôle in the lives of our fruit-eating ancestors.

On this theory of the origin and development of the colour sense, Allen concludes, (1) that individual colours are for us agreeable or disagreeable, (2) that the red and orange part of the spectrum, and also the violet end, as it tends towards red, are more agreeable than the intermediate colours, green and blue. He gives also a physiological reason for the latter conclusion, *viz.* since green and blue are the commonest colours in nature, and red and orange the most unusual, the structures of our eyes which are operative in the perception of red are less frequently exercised.[1]

Possibly the simplest criticism of Allen's theory would be a mere statement of the fact that it is not true that red and orange are more agreeable than green and blue. Individuals have, possibly, individual preferences, but no such consensus of opinion can be found to exist as Allen's theory would lead us to suppose. The question as to whether individual colours have æsthetic quality, is not of course decided thereby, but if such a quality exists, it is clear that Allen's theory gives no adequate account of it.

Wundt's view, the last to be referred to here, is in extreme opposition to those theories according to which the æsthetic value of colours is connected only with their combination. For Wundt holds that the hedonic tone (Gefühlston) is, besides intensity and quality, an essential characteristic of every sensation. Every colour-tone must

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[1] The discussion between Sully and Grant Allen carried on in the fourth volume of "Mind," may be of interest in this connection. Allen contends that colours lying near to each other in the colour circle, are unpleasant in combination because they exhaust the same class of optic fibres, and on the basis of this he enunciated a law, viz: "Pain is the result of painful stimulation." Mr. Sully writes as follows: "He (Grant Allen) hardly succeeds in including the effects of disagreeable combinations of colours under the law, viz: that 'pain is the result of painful stimulation.' The discordant juxtapositions of colours produce their effects through a successive stimulation of the same class of optic fibres which thus reaches the exhaustive and injurious point. It would follow from this that one and the same colour spread over a large surface would produce the pain of chromatic dissonance in its maximum degree." Sully thinks that the fact that this is not so shows the untenability of Allen's position. In reply Allen says that a single field of colour does produce the maximum—not of chromatic dissonance as Sully says—but of chromatic fatigue. He holds that when there is not fatigue it is because the eye is constantly wandering beyond the boundary lines.

have a particular emotional character which in its pleasantness is dependent on intensity and saturation. This emotional element varies in its transition from the different stages of animation or excitation in the red or orange, through the serene indifference of the green, to the cold and appeasing character of the blue.

If we turn now from the theoretical consideration of the æsthetic value of single colours to experimental work we must seem to be standing upon much firmer ground. The value of experiment in connection with this problem is, however, somewhat doubtful. Full reasons for this conclusion will appear in the criticism of the results reached.

Reference will be made to the work of two recent investigators, Jonas Cohn [1] and D. R. Major,[2] who deal with the hedonic tone of individual colours. So far as his work on colour is concerned, Major confines himself to individual colours, while Cohn investigates also the wider problem of the hedonic tone of colour combinations, and, so far as can be learnt, his work is the only experimental investigation of the subject, prior to that reported in this article.

Cohn used for his tests gelatine films, of which he had fifteen.[3] The observer was placed in a dark chamber and the gelatine films were interchanged in an opening in the wall of the chamber, so that the observer saw, in the case of the investigation of individual colours, only a single colour, and, in the case of the investigation of the colour combinations, two colours side by side, against a black background. For the series of experiments on individual colours he had seven observers, three of whom gave two judgments each. In his scale of agreeableness, as deduced from these experiments, while blue-green and blue are first, green and green-blue come about half way down, the reds between these, and lowest of all yellow.[4] He sums up his results on the comparative agreeableness of the individual colours in the following paragraph:

"Von zwei Nuancen derselben Farbe gefällt die gesättigtere besser. Auch unter einer Reihe verschiedener Farben werden im allgemeinen

[1] Cohn, *Experimentelle Untersuchungen uber die Gefühlsbetonung der Farben, etc.* (Philosoph. Studien, Vol X, p. 522, *et seq.*)

[2] Major, *On the affective tone of simple sense impressions.* (American Journal of Psychology, Vol. VII, p. 57, *et seq.*)

[3] For the preparation of these tests see Philosophische Studien, Vol. X, p. 573.

[4] *Ibid.* p. 574.

die gesättigteren bevorzugt. Unter annähernd gleichgesättigten Farben scheint die Bevorzugung auf rein individuellen Neigungen zu beruhen. Nur das Gelbe dürfte für die Mehrzahl hinter den anderen Farben zurückstehen, auch wenn es ganz gesättigt ist. Jedoch reichen die vorliegenden Beobachtungen nicht aus sich ein Urtheil über die Tragweite dieser letzteren Behauptung zu bilden." [1]

Major used the Milton Bradley colours, of which he had in all 137. These 137 qualities were composed as follows: "Nineteen fully saturated or typical colours, and for each of these two less saturated qualities, obtained by the inter-mixture of different degrees of black, and two obtained by the intermixture of white." These are the regular classified papers of the Milton Bradley Company. The other forty-two were made up of a number of unclassified papers which Major himself ordered. In addition to these colours he had ten different greys[2].

His method of experiment was as follows. Squares of six centimetres to the side were cut from the stimulus papers. These were exposed in two different experimental series upon card-board backgrounds of white and black respectively, beginning with the darkest shade of the colour and proceeding through the point of highest saturation to the lightest tint, or *vice versa*. This was done with all the colours in a regular order according to the spectral arrangement. No two saturations were seen at the same time. The observers sat with closed eyes two metres from the card upon which the stimulus paper was exposed and a definite time was allowed for the exposure of the colour and the recording of the judgment. Major had four observers.[3]

In comparing Major's results with Cohn's we find that the differences of the individual observers were very marked. Not only is this seen in the fact that in certain points, for example, the unpleasantness of the yellow, Major's results do not confirm Cohn's conclusion, but if we analyze Cohn's results somewhat closely we shall find marked individual differences in his own observers. For example the point of least pleasantness occurs for three of them in the violet and violet-

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[1] *Ibid.* p. 599. (Translation: Of two shades of the same colour the more saturated is the more pleasing. Also among a series of different colours the more saturated are generally preferred. If the colours are equal in saturation, the preference seems to rest purely on individual predilection. For the majority, only yellow fell behind the others even when quite saturated. However the above observations are not enough to determine the full extent of this last assertion.)

[2] See American Journal of Psychology, Vol. VII, p. 58.

[3] *Ibid.* pp. 67 and 68.

purple, for one in blue-green, and for the other three in yellow and orange-yellow. It is the *average* only that gives the result that yellow is the least pleasant, and striking an average in such a case is very clearly a doubtful method of reaching any important conclusion. Taking the combined results of Cohn and Major we find no confirmation of Grant Allen's theory that the red-orange end of the spectrum is decidedly the most pleasant, and that the blue-green region is the least pleasant.

In order to lead up to a general criticism of this experimental work on individual colours we may take the conclusions regarding yellow, and use these as illustrative of the possibilities which such a conclusion as Cohn's seems to leave out of account. It will be remembered that he found yellow the least pleasant of all the colours. But the question arises whether this aversion—if such there be—to yellow is to yellow as *such* or to yellow in certain objects and in certain combinations. Nobody dislikes yellow when endued with metallic lustre, as for instance in golden ornaments, which blend with almost any colour. The golden yellow of a field of ripe grain also gives pleasure universally. One may say that it is the metallic lustre in the one case and the motion in the other which gives the colour its beauty, but if yellow were unpleasant in itself these accessories could not make it beautiful. Yellow in its lower shades—*e.g.*, brown—is a favourite colour both in house-painting and in dress, and even in its brighter tints it is much chosen in glossy textures, such as silk.

One reason why yellow is rejected for some purposes is that when in full saturation it is the brightest of all the colours, and consequently reveals the slightest flaw and suspicion of fading. Baron Reichenbach believed that persons sensitive to the "od"[1] have a dislike to yellow. Professor Kirschmann thinks that since the discovery of the "x" rays, the theory of Reichenbach deserves renewed consideration. It may be that there are persons sensitive to rays not visible to the average person, or very much more sensitive to weak stimulations. In that case they may dislike yellow, not on account of its colour quality but on account of the intensity.

In any case the results of an investigation of the comparative agreeableness of individual colours must prove unsatisfactory for several reasons. First, there is the almost insurmountable difficulty of eliminating the influence of previous impressions and associations

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[1] The "od" is supposed to be a fine ethereal matter radiating from graves and from the poles of magnets, etc.

with special objects, situations, etc. Secondly, it is impossible to judge of one colour unless the whole field of vision be filled with it.[1] In both Major's and Cohn's experiments there was a combination with colourless light (either black or white). But even if the whole field of vision be filled with one colour, the judgment is not yet made on the affective merits of the colour quality alone, but possibly on the extension as well. For example, for many persons purple-red is a favourite colour when used in limited extension, but scarcely anybody would like a red which would fill the whole field of vision. The spacial extension is certainly not irrelevant. Large surfaces of yellow, green and blue are pleasing, but the colours of the ends of the spectrum, especially red, orange and purple, are usually found to be pleasing only in limited extension. This may, however, be the result of adaptation, since nature is very prodigal in the colours of medium refrangibility (yellow, green, and blue), and rather sparingly throws in the spots of red, violet and purple.

II

HISTORY OF THE PROBLEM—*Continued*

THE ÆSTHETIC VALUE OF COLOURS IN COMBINATION

A much more important problem than the æsthetic value of isolated colours is that of the æsthetic value of colours in combination.[2] The majority of writers who have expressed opinions on this subject hold that complementary colours make the most pleasant combinations. Alberti and Da Vinci, artists of the Renaissance, were of this opinion. Alberti says, "Pleasure will arise when a colour is strongly differentiated from the one which stands beside it." He advocates the juxtaposition of bright and dark colours. Da Vinci thinks that there should be a contrast, both of colour tone and intensity.[3]

The reason, according to Chevreul, that complementary colours make a pleasant combination is because being so far removed from

[1] It is not here forgotten that the retina is not equally sensitive at all points for different colours, but as Kirschmann has noticed in his article on the subject, if the field of vision be filled objectively with the same colour, no differences are noticed. (*Die Helligkeits-Empfindung im indirecten Sehen*, Phil. Stud. V, p. 478.)

[2] As the works of several of the anthors to whose opinions reference is made were not accessible to the writer, their theories are taken to be as stated in the article of Jonas Cohn already cited.

[3] Cohn, *Experimentelle Untersuchungen, etc.*, chap. 3. (Philosoph. Studien, vol. X.)

each other, they cannot have a mutually injurious effect.[1] If the colours though not complementary are so far apart that they cannot injure each other, the combination is still pleasant.[2] Colours near together in the colour circle may make a pleasant combination, but it is exceedingly rare to find a combination which is not either reciprocally injurious or, at least, injurious to one of the two combining colours. If we place, for example, a blue near to a green, the blue appears to approach violet and the green yellow. Hence arises what is called the damaging contrast discovered by Chevreul. Such combinations he thinks bad. Chevreul notices that the effect of the combination is modified by the gloss of the surface and still more by the form.[3] As an example of the former he gives the effect of gloss in the plumage of certain birds. This gloss, or metallic lustre, so affects the combination that it makes pleasant what might otherwise be an unpleasant combination. As examples of the effect of form he refers to flowers. There are certain combinations which would be objectionable in plane surfaces, but as seen in flowers are decidedly pleasant.

Grant Allen's views on combinations of colours have already been quoted. Special attention, however, must be paid to his statements that colours lying near together in the colour circle do not harmonize, and that the combinations of complementary colours are the most agreeable.

Sully lays down the following rules concerning colour combinations: (1) Combinations which seem to be pleasant include both wide and narrow intervals. (2) In combining colours for the purpose of investigating the æsthetic value of their combination we must give them an equal degree of brightness, for if one colour is much darker than another the contrast may please through the contrast of light and dark. (3) The presence of a third colour affects the degree of affinity of the two colours.[4] In connection with the first rule, he gives the following reason why complementary colours are the most agreeable. It is, he says, because such combinations give a sense of completeness, for we need unity as well as variety. He thinks that the same sense of completeness is found in the trials—red, yellow and blue; orange, green and violet. In this explanation it will be noticed that the emphasis is placed upon unity. It is evident that in all

[1] Chevreul, *De la Loi du Contraste Simultané des Couleurs*, p. 465, par. 857.

[2] *Ibid.*, p. 84, par. 178.

[3] *Ibid.*, p. 463, par. 854.

[4] Sully, *Harmony of Colours*. (Mind, vol. IV., p. 172.)

colour combinations variety exists, hence variety—for the mere fact that there is a colour combination—is not the only factor necessary to an agreeable combination. The question then comes to be whether this "completeness" or "unity," which Sully finds in the combination of complementary colours, is something which inheres in the sensations. To this the results of our experiments give the unequivocal answer, no; for, as will be seen by reference to them, the combination of complementary colours is by no means the most pleasant. If this "completeness" or "unity" does not inhere in the sensations, is it not probable that Sully is speaking in the light of his knowledge that complementary colours are those which, when mixed in certain proportions, give colourless light? If this is the unity to which he refers it is clear that the combination of complementaries will be necessarily agreeable only to those who know the physical fact.

Brücke says that small intervals please if the distribution of intensity and the difference in quality correspond to the natural effect of shadow. An example may illustrate the meaning of the proposition. The quality of light is not perfectly independent of the intensity. If we change the intensity of a colour considerably we change, at least slightly, the quality also. Increase of luminosity causes the colour tone to shift towards the orange-yellow, the most intensive of the constituents of white light; decrease of brightness, on the other hand, shifts the colour tone somewhat in the direction of the violet-blue, the darkest region of the spectrum or colour-circle. This is the so called phenomenon of Purkinje. If, then, a shadow be cast on a red surface, the shadow, though still red, will be a rather purplish red. A strong light thrown on the same surface, on the other hand, will give it a tendency towards the orange. Now Brücke's idea is that in combining similar colours we must take our selection in analogy with the above facts. If we wish to combine a red with a neighbouring but brighter colour, it must be an orange-red or orange. But if the other component is to be darker, we ought to choose it on the purplish side, *i.e.*, a reddish purple or purple.

Von Bézold correctly holds that the greatest beauty is reached, not with complementary colours, which he finds harsh, but with colours a little removed from the complementary. He says that combinations at medium intervals are unpleasant; first, because of their undetermined nature,—they show neither approaching similarity nor sufficient opposition—and secondly, because of contrast.[1]

[1] Cohn, *Experimentelle Untersuchungen, etc.*, chap. 3, (Philosoph. Studien, vol. X.)

Alfred Lehmann in his *Elementary Colour Aesthetics* (published in Danish[1]) lays down two laws, of equivalence and of induction. Two combining colours should hold the attention with equal strength. This is accomplished by giving the less saturated colour a greater extension. Lehmann has presented for proof of this law twenty trials according to the "Herstellung" method. He has, says Cohn, opened up the way for further investigations, but the number of trials was not sufficient to form a basis for valid conclusions.

Wundt[2] lays due emphasis on the importance of the affective character of the components as an element in the æsthetic value of the combination, the difference of the quality being a second element. With respect to the latter point he says: "Eine unbefangene Beobachtung muss jedoch in dieser Beziehung wohl bei der Bemerkung stehen bleiben dass contrastierende Farben in ihrer sinnlichen Wirkung sich heben, verwandte Farben aber verschiedene Abstufungen einer in ihrem Grundcharakter übereinstimmenden Wirkung hervorbringen." That is, contrasting colours add mutually to one another in their sensuous effects, while closely related colours please by producing effects which agree in their fundamental æsthetic character. Wundt, however, adds that this rule is far from having a determining value for colour combinations, since the æsthetic value of these depends so much also on the sensuous effect of the single colours, and on the relations which we are accustomed to see in nature. This view is in agreement with the results reached in our investigation as long as it is kept in mind that the terms *contrasting* colours and *complementary* colours are by no means identical. When the difference in quality of two colours has reached its maximum, as in the case of fully saturated complementary colours, this difference can manifestly not be increased by contrast.[3] Consequently complementary colours are properly speaking not contrasting colours, although each of them induces the other in colourless surfaces.

Professor Kirschmann thinks with Von Bézold that the most pleasant combinations will be found with colours not at, but near the complementary. The eye demands contrast, and is hurt by lack of variety and change. He also strongly holds the view that the extension of the surface is by no means unimportant. Colour theories have hitherto failed to evolve rules of any value for the combination of

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[1] *Favernes Elementaere Æstetik.*

[2] Wundt, *Physiologische Psychologie* (4th ed.), vol. II., p. 237, *et seq.*

[3] Cf. Kirschmann, *Ueber die quantitativen Verhältnisse des simultanen Helligkeit- und Farbencontrastes.* (Philos. Studien, vol. VI. p. 486).

colours, just because of the great number of variations and combinations possible. There should, he says, always be taken into account variability of tone, of saturation, of intensity, of shape, and of size; and he insists that when all these variables are taken into consideration any two colours can be made to combine harmoniously, and that until we have tried all the possibilities of any two colours, we cannot say that they will not blend.[1]

For the history of experimental work on the æsthetics of colour combination, the paper of Jonas Cohn, already referred to, is the principal authority. Although the experimental investigation of colour combinations is of comparatively recent introduction, the investigation of combinations involving other variables dates back to the time of Fechner.

Different theories had been advanced concerning the comparative pleasurableness of various forms. Home (Lord Kames), for instance, had said that a square is more beautiful than a parallelogram

[1] Kirschmann, *On the Æsthetic Significance of Light and Colour Contrast*, (Philos. Studien, vol. VII., p. 391). In his lectures on psychological optics, Professor Kirschmann makes the following propositions as provisional rules until something more definite has been established on the basis of a thorough analysis of the facts:—

(1) Our sense of sight seems to find pleasure in combinations (of two components) with a maximum, or at least a great contrast effect. It must be noticed here that the greatest contrast effect is not reached by the greatest differences.

(2) The contrast in operation may be either that of colour, saturation or light (intensity).

(3) It is not in the combination of complementary colours that we have the best effects but in those which, through contrast, are brought nearer to the appearance of complementaries.

(4) Combinations of different saturation degrees (practically shades and tints) of the same colour or of very similar colours are also found to be pleasant.

(5) In cases where colour contrast and saturation contrast are absent or weak, the intensity contrast may replace them.

(6) Colourless light harmonizes with every colour if chosen of the right intensity.

(7) *For any two qualities of the colour circle there can be found degrees of saturation and brightness in which they will form an agreeable combination.*

(8) Colours of high saturation and of equal brightness (complementary colours not excepted) when combined do not make combinations of marked agreeableness.

(9) The absolute and relative extension and the space configuration (shape, location, etc.) of the components have a great effect on the pleasurableness of the combinations.

(10) The hedonic tone of the components is a factor not to be neglected when considering the æsthetic effect of a colour combination.

because more regular, and a circle than a square because more simple.[1] Fechner maintained that the actual pleasurableness of these various forms must be tested, and he set about devising means of doing so. Three methods of investigation were distinguished by him as follows: (1) *The Statistical Method*, by which account is taken of several combinations in actual use, *e.g.*, those found in a certain number of drawing-rooms. This, as has been shown by Cohn, is not an experimental method, for the most important characteristic of experiment, the systematic change of the conditions, is lacking. (2) *The Method of Production*, according to which the observer is asked to produce a pleasant combination. (3) *The Method of Choice.* The second is really a method of choice as well as the third, for the observer in producing a pleasant combination is, in fact, exercising choice. The method of choice is the one employed by Fechner himself. Witmer[2] varied the method by using two figures only at a time. This is what Cohn calls the method of comparison by pairs and is the method which he himself applies to the combination of colours.

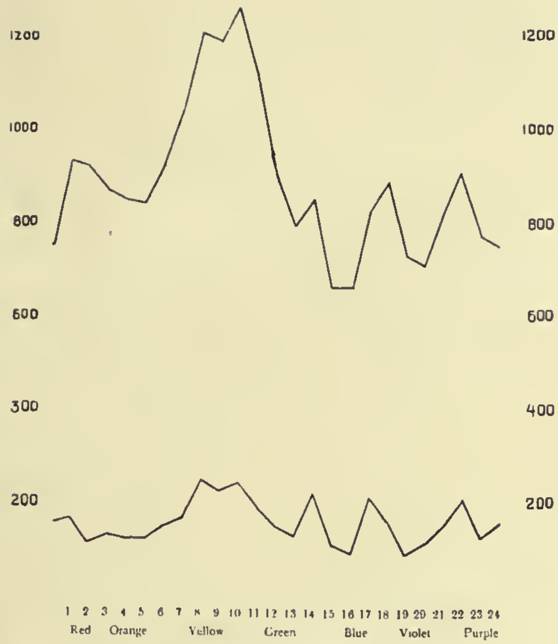
Cohn made two series of experiments, the first with pigment colours, the second with gelatine preparations. In the first series of experiments he took ten pigment colours, representing ten positions on the colour circle. These colours were chosen with special reference to the complementaries.[3] He placed before the observer two combinations—for example green with blue, and green with orange. The observer was then asked which of the combinations was the more pleasant. As each colour was combined with every other colour there would be possible $\frac{10 \times 9}{2} = 45$ combinations. He had six observers. The results of his observations were represented by a curve. He found that the combinations were more pleasing in regular progression as the colours approached the complementary relation.[4] It is not easy to understand how he got so regular a curve unless that with such a small number of colours, and these chosen with special reference to the complementaries, the complications which might have arisen from the use of a greater number of transitional colours were avoided.

[1] Lord Kames, *Elements of Criticism*, ch. 3: On Beauty.

[2] Philos. Studien, vol. IX., p. 96, *et seq.*

[3] It is not evident how Cohn determined the complementary relation of his colours.

[4] Phil. Studien, vol. X., p. 574.



XXXVII

Plate 15 COMBINATION-CURVE

In Cohn's second series of experiments instead of using pigments he used gelatine preparations. This part of Cohn's work is referred to above (p. 32) in regard both to the gelatine preparations and also to the method of experiment. In these experiments the observer saw two colours at a time and was asked to judge as to the pleasantness of the combination. The results of this series of experiments, like those with pigments, show that the greatest possible difference is what pleases best.

Cohn claims certain advantages for this method over the experiments with pigments—(1) the darker chamber can be employed, (2) the observer is freed from the experimenter's judgment, (3) pure colours are used. As to the purity of his film preparations, his table does not give a clear statement.[1] He does not say for what aperture of the slit of the spectroscope his notation of the wave lengths is valid. If we compare the astonishingly small regions of the spectrum which he claims to isolate, sometimes by not more than two films, with the gelatine combinations described by the originator of this method of producing comparatively monochromatic light, we must conclude either that Cohn's gelatine films were of an absorbing power far superior to those employed by Kirschmann, or that he used for his spectroscopical examination such an extremely narrow aperture of the slit that it would but poorly correspond to the intensity of light to which the gelatine preparations were subjected in Kirschmann's experiments.[2]

A second criticism that can be made on Cohn's experiments is that he had obviously no means of excluding differences of intensity. Further, the relative agreeableness of the individual colours should not have been taken until after the judgments on the combinations had been given. There is in every judgment on a combination not only the affective tone of the combination but combined with this the affective tone of each of the individual colours. To call attention to the individual colours first gives the affective tone of these too prominent a place in the judgment. It would seem also that this method could not furnish a definite scale of the agreeableness of the combinations, for these were never compared with one another, there being only one combination before the observer at a time. Thus the observer, although expected to give an absolute judgment on the combination before him, was subject to all the influences which spring

[1] *Ibid.*, vol. X., p. 574.

[2] Kirschmann, *Ueber die Herstellung monochromatischen Lichtes.* (Philos. Studien, vol, VII., p. 543 *et seq.*)

from the succession in which the combinations were shown. The errors resulting from this circumstance could only be avoided by exhausting all the possible variations in the succession, a task which could, of course, never be carried out.

Miss H. B. MacDougall performed some experiments in the Psychological Laboratory of the University of Toronto on the æsthetic value of colour combinations. She used Prang's standard pigment colours mounted on pieces of card-board 83 mm. by 33.5 mm. Instead of taking ten colours, as did Cohn, she took twenty-four. With each of the twenty-four was combined each of the other twenty-three in turn. All the colours were placed upon a table covered with black velvet and beside each of these was placed the constant, or combining colour. The observer was asked to pick out from among all the combinations before him those which he found pleasant, and from among these the most pleasant. The same process was followed in the case of all the twenty-four colours. The number of combinations was thus $\frac{24 \times 23}{2} = 276$, just six times as many as Cohn employed.

In the report of her year's work Miss MacDougall, after referring to the comparatively small number of experiments made, nevertheless finds the following conclusions warranted:—(1) Individuals differ in their judgments of the agreeableness and disagreeableness of colours; and (2) the judgments of the same individual, at different times, do not vary to any great extent.

In relation to Miss MacDougall's conclusions it may be noted here by anticipation, that her second conclusion is fully supported by the results of the experiments of the writer, and that the first conclusion is open to the same criticism as the experimental work of Cohn and Major, *viz.*, no such general conclusion can be drawn from the experiments of a small number of individuals. Though diversity may be proved from a few observers, a fundamental agreement with regard to certain tendencies of preference (in spite of diversities) can only reveal itself with a large number of observers.

III

EXPERIMENTAL WORK

The problem at which Miss MacDougall had been working was subsequently taken up by the writer. As before stated, Prang's standard colours were used. With some other investigators the Milton Bradley colours have been more in favour as they are in some cases more saturated than the Prang colours, notably in some of the so-called principal colours, as blue and orange. For many purposes the Milton Bradley colours are preferable, but for our investigation a series of colours was needed in which no individual colours possessed a special attraction on account of greater brilliancy or saturation.

The procedure in the experiments was the same as that followed by Miss MacDougall;[1] but instead of only one observer at a time, as in Miss MacDougall's series, from three to five were employed. It thus became necessary to enforce very strictly the rule that there should be no communication regarding the experiment among the observers; slips of paper were prepared in which each observer recorded his judgments according to a regular method. As the value of all such experiments must depend upon the possibility of securing the observer's unbiassed judgment in the combinations presented, it will at once be seen that by these precautions we avoided the possible difficulty mentioned by Cohn[2] of the observer being prejudiced by the judgment of others.

Another difficulty to which both Cohn and Major have called attention is the dependence of the judgment of the individual upon his 'mood,' which means that the simple sense impression would not in such cases determine the judgment. A mood in a person indicates essentially the predominance of a certain class of associated factors in the presentations in consciousness at the time. What does not seem to be adequately realized is that there is no essential difference between the associations in which colour combinations stand and the influence of varying moods upon the judgment. A mood will have an influence in changing one's judgment regarding the agreeableness of colour combinations only in so far as the associated elements are taken into account, or are operative.[3] It is to be noticed, then, that

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[1] See p. 42.

[2] Philos. Studien, vol. X, p. 571, *et seq.*

[3] The term 'mood,' though often used, does not seem to have been defined. It is generally used to express only some outstanding condition of consciousness, such as discouragement, joyfulness, despondency, etc. In so far as the term 'mood' refers merely to a condition of consciousness, without any reference to that condition being somewhat striking, or out of the ordinary, it is obvious that the condition of consciousness can always be called a mood. To do so, however, is to make the term quite meaningless. The fact that there are peculiar conditions of consciousness, such as those noticed, evidently does not mean that consciousness is, so far as its general laws are concerned, one whit different from the ordinary experiences. Therefore the moods have simply to do with special contents of consciousness which are inter-related, or associated with other elements exactly as the contents of consciousness are always associated.

great variations in individual judgment, or, what is the same thing, lack of uniformity in the judgment given by the same individual at different times, are to be attributed to the presence, or more properly speaking the predominance of associated factors in the judgment. It would seem as if these associational factors are much more likely to predominate in judgments of individual colours than in judgments of colour combinations. Still, a strong prejudice with regard to an individual colour will have a certain influence on the judgment of any combination in which that colour occurs. A striking illustration of this occurred in the case of one of our observers, a young Greek, who found among the colours used the green which is the colour of which the Turk's turban is made. He not only disliked the colour itself, but he found almost all the combinations disagreeable in which that colour occurred.

With these difficulties in mind we were careful to ask the observers to let the simple sense impression determine their judgment, that is, to say only whether the combinations as submitted were pleasant or unpleasant. To carry out these instructions involves in an observer several conditions which, however much desired by the observer himself, are not easily attained. Among them two may be mentioned, (1) the absence of all preconceived theories as to which colours make pleasant combinations, and (2) the disregard of all questions of utility in respect to colour combinations, for example the association of the combinations with dress, decorations, etc. It is evident that an observer would not be at all likely to make use of his theory, or to associate the combination with such objects as those mentioned, without being conscious of doing so, therefore the desired conditions may be reached if the observers are really careful in their work. The difference between these conditions and that already noticed in the case of one who habitually dislikes a certain colour—through whatever experiences this condition has been reached—is decidedly marked. No amount of care can at a moment's notice eliminate such a dislike, as the case of the Greek, already mentioned, will shew. Among our observers there was found this single case only of marked dislike, and the curves which follow will shew very clearly that the observers must have eliminated very largely the other factors from their judgments, or there could never have been such agreement as is indicated by the sharply decided and constant maximal and minimal points.

The room in which the experiments were conducted was lighted altogether from above, and was in a part of the building where there were no distracting influences. The nature of the lighting (by sky-light) made it possible to have all the observers close enough to the table on which the colours were placed without casting any shadows. For convenience we designated the colours by numbers. By reference to Table I it will be seen to what colours these numbers correspond. In the first column will be found the number as used by us, in the second the corresponding designations employed by Prang, and in the third the names which are commonly applied to the colours. Each colour was combined successively with all the others, and in designating the combination the number given represents the number applied by us to the colour with which the constant colour is for the time being combined.

The combinations were placed in two rows, (number 1 being at the left-hand end of the upper row with 24 immediately below it), on a table covered with black velvet. Each combination, which covered an oblong of 67 mm. by 83 mm., was separated from the next by an interval of 100 mm. In order to avoid mistakes, small numbers of white metal were fastened to the velvet at the head of the combinations. There were in all thirty observers, consisting of professors, the wives of professors, and men and women students from the different departments of the University. There were also a few ladies not connected with the University.

Each of the observers underwent the series of experiments twice. This gave $30\times2\times2=120$ judgments on each combination; and as there were (as before stated) $\frac{24 \times 32}{2}=276$ combinations, this gave in all $276\times120=33{,}120$ combinations judged, or 1,440 complete trials, each trial representing the choice of pleasant and most pleasant out of 24 combinations. This investigation was pursued under the direction of Professor Kirschmann, to whom especial thanks are due for much valuable assistance. Acknowledgment should also be made here to Mr. A. H. Abbott, B.A., Assistant in the Psychological Laboratory, for the help with which he favoured us from time to time, and also to the ladies and gentlemen above mentioned for their painstaking work and the extreme care which they took in following the instructions given.

TABLE I

Numeral Designation.	Prang's Designation.	Names of Colours Used by us.
1	R. (red)	Purplish red.
2	R. R. O. (red red orange)	Red.
3	R. O. (red orange)	Orange red.
4	O. R. O. (orange red orange)	Reddish orange.
5	O. (orange)	Orange.
6	O. Y. O. (orange yellow orange)	Yellowish orange.
7	Y. O. (yellow orange)	Yellow orange.
8	Y. Y. O. (yellow yellow orange)	Orange yellow.
9	Y. (yellow)	Yellow.
10	Y. Y. G. (yellow yellow green)	Green yellow.
11	Y. G. (yellow-green)	Yellow green.
12	G. Y. G. (green yellow green)	Yellowish green.
13	G. (green)	Green.
14	G. B. G. (green blue green)	Blue green.
15	B. G. (blue green)	Green blue.
16	B. B. G. (blue blue green)	Blue.
17	B. (blue)	Violetish blue.
18	B. B. V. (blue blue violet)	Violet blue.
19	B. V. (blue violet)	Blue violet.
20	V. B. V. (violet blue violet)	Violet.
21	V. (violet)	Purplish violet.
22	V. R. V. (violet red violet)	Purple violet.
23	R. V. (red violet)	Violetish purple.
24	R. R. V. (red red violet)	Purple.

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	
1		4	6	15	18	27	22	34	28	37	32	29	29	26	32	20	19	20	23	26	19	11	5	1	483
2	4		2	3	14	22	25	29	28	40	34	31	33	26	26	24	17	29	22	27	24	12	6	2	480
3	8	5		5	10	21	23	30	26	34	36	26	32	28	28	17	17	28	26	29	22	11	8	3	473
4	11	6	7		9	12	18	25	18	31	30	26	24	25	27	20	20	31	26	22	18	17	19	12	454
5	17	10	9	7		5	15	14	12	22	26	22	20	21	25	21	21	24	27	24	23	28	16	16	425
6	27	19	8	12	8		2	8	15	18	17	16	19	20	26	25	24	27	29	33	31	27	24	23	458
7	23	25	17	15	9	4		6	11	18	20	25	24	27	25	29	34	27	33	33	34	30	35	24	528
8	37	33	28	20	13	6	5		8	11	28	31	32	34	33	35	39	40	34	39	33	36	38	32	645
9	35	32	31	30	21	9	7	6		7	25	33	33	36	38	35	39	42	36	39	39	34	37	30	671
10	38	36	38	31	31	28	24	20	17		17	24	29	34	30	25	26	33	35	38	37	37	39	37	704
11	37	35	38	30	28	25	31	27	29	15		14	15	19	14	19	17	19	20	26	34	29	33	29	583
12	26	29	26	25	25	32	36	33	28	23	11		4	8	4	14	9	14	14	12	15	21	23	25	457
13	19	35	19	20	19	24	28	26	25	26	14	7		4	1	5	7	9	9	7	10	6	16	20	356
14	27	29	28	19	27	28	31	27	27	23	22	14	6		6	9	10	9	12	5	13	16	20	23	431
15	14	18	12	11	8	14	16	17	23	12	17	4	3	0		3	5	5	6	6	5	17	15	9	240
16	14	12	18	20	11	20	24	24	18	20	15	6	3	1	0		4	1	4	4	6	16	9	13	263
17	21	22	22	20	21	33	34	31	28	31	24	17	15	11	11	10		4	6	13	13	13	20	19	439
18	30	24	23	23	20	31	31	35	30	29	26	13	16	14	16	11	7		3	6	12	9	18	21	448
19	18	13	14	15	21	19	26	23	22	21	16	12	11	5	4	5	7	5		2	6	3	15	11	294
20	10	15	11	15	19	18	24	26	24	24	12	8	9	6	3	3	2	4	0		6	12	10	9	270
21	15	18	20	17	25	25	25	30	30	29	27	18	15	14	15	10	10	12	10	4		8	9	16	402
22	15	13	20	26	32	28	30	37	34	36	34	30	29	22	19	16	16	15	16	14	8		6	20	516
23	5	6	5	9	17	18	23	26	21	25	26	19	18	18	18	13	14	18	18	17	7	5		6	352
24	2	2	2	9	15	16	15	24	19	27	26	24	23	21	21	18	18	20	21	20	16	4	4		367
	453	441	404	397	421	465	515	558	521	559	535	449	442	420	422	387	382	436	430	446	431	402	425	401	10,742

TABLE III—Pleasant Combinations

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	
936	8	14	26	35	54	45	71	63	75	69	55	48	53	46	34	40	50	41	36	34	26	10	3	1
	921	7	9	24	41	50	62	60	76	69	60	68	55	44	36	39	53	35	42	42	25	12	4	2
		877	12	19	29	40	58	57	72	74	52	51	56	40	35	39	51	40	40	42	31	13	5	3
			851	16	24	33	45	48	62	60	51	44	44	38	40	40	54	41	37	36	43	28	21	4
				846	13	24	27	33	53	54	47	39	48	33	32	42	44	48	43	48	60	33	31	5
					923	6	14	24	46	42	48	43	48	40	45	57	58	48	51	56	55	42	39	6
						1043	11	18	42	51	61	52	58	41	53	68	58	59	57	59	60	58	39	7
							1203	14	31	55	64	58	61	50	59	70	75	57	65	63	73	64	56	8
								1195	24	54	61	58	63	61	53	67	72	58	63	69	68	58	49	9
									1263	32	47	55	57	42	45	57	62	56	62	66	73	64	64	10
										1118	25	29	41	31	34	41	45	36	38	61	63	59	55	11
											906	11	22	8	20	26	27	26	20	35	51	42	49	12
												798	10	4	8	22	25	20	16	25	35	34	43	13
													851	6	10	21	23	17	11	27	38	38	44	14
														662	3	16	21	10	9	20	36	33	30	15
															650	14	12	9	7	16	32	22	31	16
																821	11	13	15	23	29	34	37	17
																	884	8	10	24	24	36	41	18
																		724	2	16	19	33	32	19
																			716	10	26	27	29	20
																				833	16	16	32	21
																					918	11	21	22
																						777	10	23
																							763	24

21,484 = 2 × 10,742

TABLE IV—Most Pleasant Combinations

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	
1		0	0	1	4	5	2	3	2	3	7	7	13	10	6	2	5	4	6	4	2	2	0	0	88
2	0		0	0	2	0	1	2	5	2	4	2	4	5	2	2	1	1	3	2	3	1	0	0	42
3	0	0		0	0	3	2	3	1	3	3	4	5	6	5	1	1	2	4	3	2	1	2	1	52
4	1	3	0		1	1	1	1	2	4	3	3	6	1	3	3	4	1	2	2	2	4	0	2	50
5	2	1	1	1		1	0	2	0	0	1	5	0	1	3	2	4	5	3	3	5	3	1	0	44
6	3	2	4	2	1		0	0	3	0	3	3	2	3	2	5	2	3	1	3	2	4	2	2	52
7	6	3	2	1	2	0		2	0	0	5	3	4	7	2	7	2	2	3	6	3	2	2	2	66
8	7	4	5	2	3	2	1		2	1	2	7	7	8	13	10	13	14	10	10	12	12	14	8	167
9	5	4	2	4	0	1	1	1		1	5	9	13	9	13	11	10	11	9	8	10	7	4	6	144
10	13	9	11	6	6	3	1	2	1		2	4	3	6	7	5	6	9	12	12	13	13	13	8	165
11	8	6	7	4	3	1	5	2	4	1		2	4	3	4	5	2	2	6	6	11	8	6	8	108
12	5	5	4	8	4	6	3	3	2	2	2		0	2	0	0	2	2	2	1	1	3	6	5	68
13	1	3	2	3	4	4	3	4	2	1	0	4		3	1	1	2	1	0	1	0	0	1	6	47
14	10	11	13	7	10	10	9	6	11	4	4	3	2		2	5	4	4	3	2	3	7	5	8	143
15	3	4	2	0	1	0	0	0	2	0	0	0	0	0		1	2	0	1	1	1	0	4	4	26
16	0	1	1	2	0	2	0	1	2	3	1	1	0	1	0		0	1	0	0	1	1	3	1	22
17	10	7	10	7	10	12	14	12	10	12	8	5	4	0	3	1		2	1	0	0	1	3	6	138
18	2	4	3	4	6	7	11	9	5	10	6	2	1	1	1	3	1		1	2	1	3	3	4	90
19	1	2	1	0	2	1	1	2	5	1	0	C	0	0	0	0	1	0		0	0	2	0	0	19
20	0	2	0	2	1	2	0	6	2	4	0	0	1	1	2	1	0	1	0		1	2	2	1	31
21	3	1	4	7	3	8	6	5	5	4	4	5	2	4	2	1	0	3	3	1		2	1	2	76
22	2	3	5	11	12	7	11	10	16	16	8	2	2	2	3	1	1	3	2	3	2		3	4	129
23	0	1	1	2	2	0	2	2	2	4	5	6	4	1	2	3	1	1	3	3	1	0		3	49
24	2	0	0	2	3	4	2	6	1	4	8	6	4	8	5	4	5	3	3	3	3	1	0		77
	84	76	78	76	80	80	76	84	85	80	81	83	81	82	81	74	69	75	78	76	79	79	75	81	1893

TABLE V—Most Pleasant Combinations

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	
172	0	0	2	6	8	8	10	7	16	15	12	14	20	9	2	15	6	7	4	5	4	0	2	1
	118	0	3	3	2	4	6	9	11	10	7	7	16	6	3	8	5	5	4	4	4	1	0	2
		130	0	1	7	4	8	3	14	10	8	7	19	7	2	11	5	5	3	6	6	3	1	3
			126	2	3	2	3	6	10	7	11	9	8	3	5	11	5	2	4	9	15	2	4	4
				124	2	2	5	0	6	4	9	4	11	4	2	14	11	5	4	8	15	3	3	5
					132	0	2	4	3	4	9	6	13	2	7	14	10	2	5	10	11	2	6	6
						142	3	1	1	10	6	7	16	2	7	16	13	4	6	9	13	4	4	7
							251	3	3	4	10	11	14	13	11	25	23	12	16	17	22	16	14	8
								229	2	9	11	15	20	15	13	20	16	14	10	15	23	6	7	9
									245	3	6	4	10	7	8	18	19	13	16	17	29	17	12	10
										189	4	4	7	4	6	10	8	6	6	15	16	11	16	11
											151	4	5	0	1	7	4	2	1	6	5	12	11	12
												128	5	1	1	6	2	0	2	2	2	5	10	13
													225	2	6	4	5	3	3	7	9	6	16	14
														117	1	5	1	1	3	3	3	6	9	15
															96	1	4	0	1	2	2	6	5	16
																207	3	2	0	0	2	4	11	17
																	165	1	3	4	6	4	7	18
																		97	0	3	4	3	3	19
																			107	2	5	5	4	20
																				155	4	2	5	21
																					208	3	5	22
																						124	3	23
																							158	24

3786 = 2 × 1893

In Table I we give the colours used and their designation. Tables II to V inclusive represent the tabulated results of all these experiments, Tables II and III representing the pleasant combinations and Tables IV and V the most pleasant. In Table II the numbers at the top in horizontal order represent the 'combining colours.'[1] Those at the left in vertical order represent the colours which were combined with the 'combining colours.' Thus when 1 was the 'combining colour' it was chosen four times with 2, eight times with 3, eleven times with 4, etc., as a pleasant combination. Table IV is analogous to Table II, representing the most pleasant combinations.

Each of these combinations appeared twice; for example, 1 appeared with 3 once, when 1 was the 'combining colour' and again when 3 was the 'combining colour,' it being chosen 8 times in the first instance and 6 times in the second as a pleasant combination.[2] In Tables II and IV the two cases are expressed separately, the former being found in the lower triangle (to the left), the latter in the upper triangle (to the right). In Tables III and V they are combined; consequently each number in those tables indicates how often the combination of the colours found at the end of the vertical and horizontal series which cross each other there was chosen.

In Tables II and IV each of the sums of the vertical columns represents the total number of judgments of 'pleasant' and 'most pleasant' respectively when the colour, indicated by the number at the head of the column, was the 'combining colour,' whereas the sums of the horizontal series give the total when the colour indicated by the number at the opposite end of the line was combined with each of the others respectively. The number at the lower right-hand corner of each table gives the totality of the combinations selected.

In Tables III and V each of the numbers at the diagonal indicates the sum total of all the judgments for the colour whose number is found at the head of the column and also at the right-hand end of the horizontal. For example 1043 indicates the sum total of all the judgments of 'pleasant' for the colour 7.[3]

Just here we should like to notice how the space error eliminated itself. We have already referred to the fact that each combination appeared twice. The 'combining colour' was always placed as the

[1] By 'combining colour' is meant the colour which in each of the 24 combinations that constituted one complete trial was the same.

[2] See Table II.

[3] See Table III.

right-hand member of the combination; therefore each colour appeared once on the right and once on the left of the combination, on the right when it was the combining colour, on the left when it was combined with any other.

Although these tables give a complete statement of the results in the smallest possible space they do not permit us to take in at a glance the chief characteristics; therefore, in order that this may be done, these results have been expressed in curves,[1] representing the results as shown in Tables III and V. The Roman numerals by which the curves are designated correspond to the numbers of the colours. Thus, Curve I represents the results of all the combinations in which the colour 1 took part, Curve II the results of those in which the colour 2 took part, etc. The upper curve represents in each case the pleasant combinations, the lower the most pleasant.

The abscissa line represents the twenty-four colour intervals in spectral order. The length of the ordinates indicates the frequency with which each colour was chosen as a pleasant and most pleasant combination respectively with the colour indicated by the number of the curve. Thus the height of the ordinates in any of the curves is taken directly from Tables III and V respectively, which will readily be understood by reference to those tables.

[1] Curves I to XXIV.

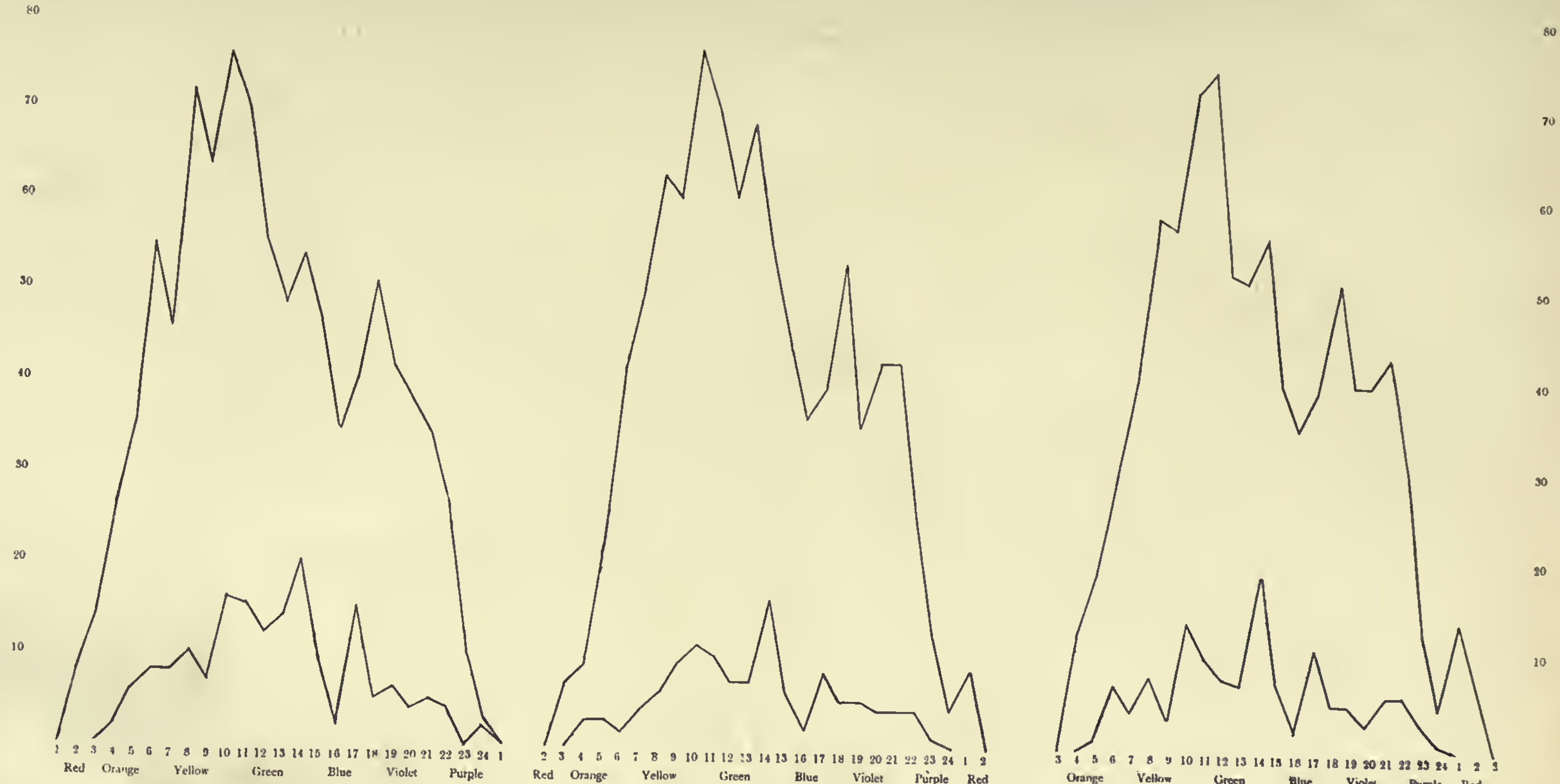
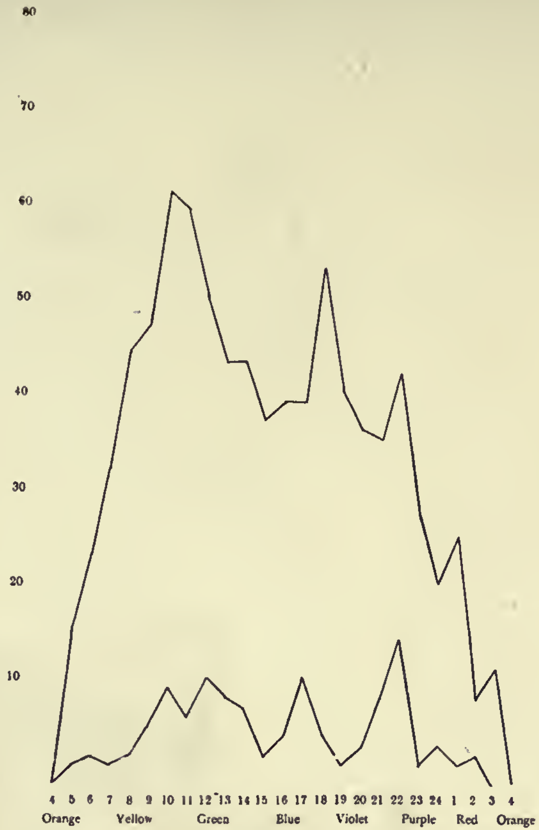


Plate I

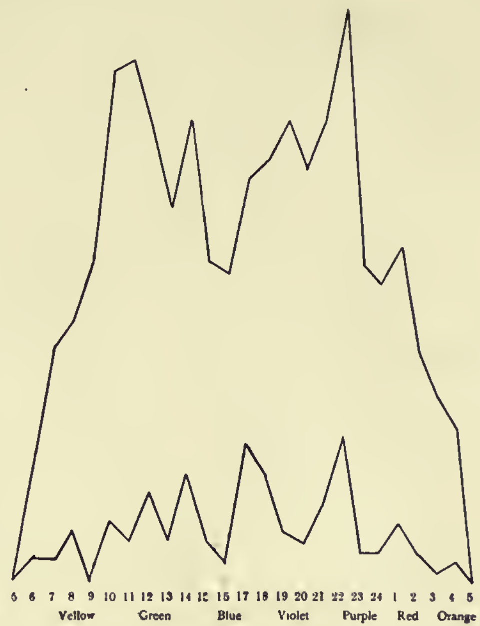
I
PURPLISH RED

II
RED

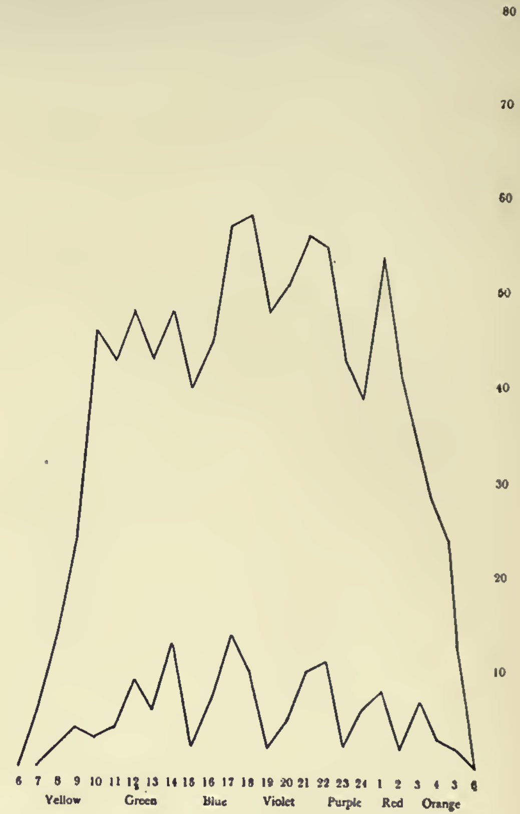
III
ORANGE RED



IV
REDDISH ORANGE

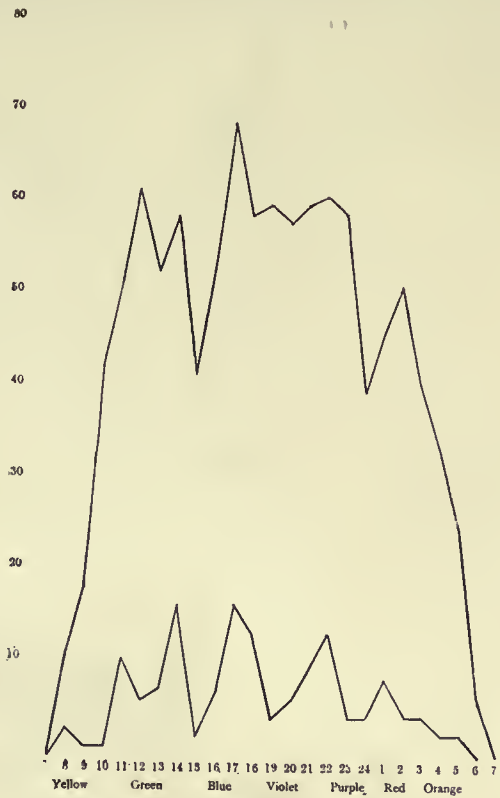


V
ORANGE



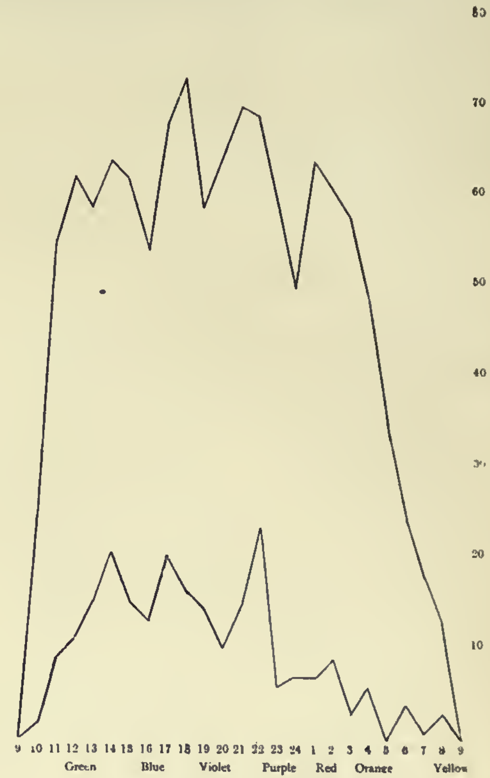
VI
YELLOWISH ORANGE

Plate 2



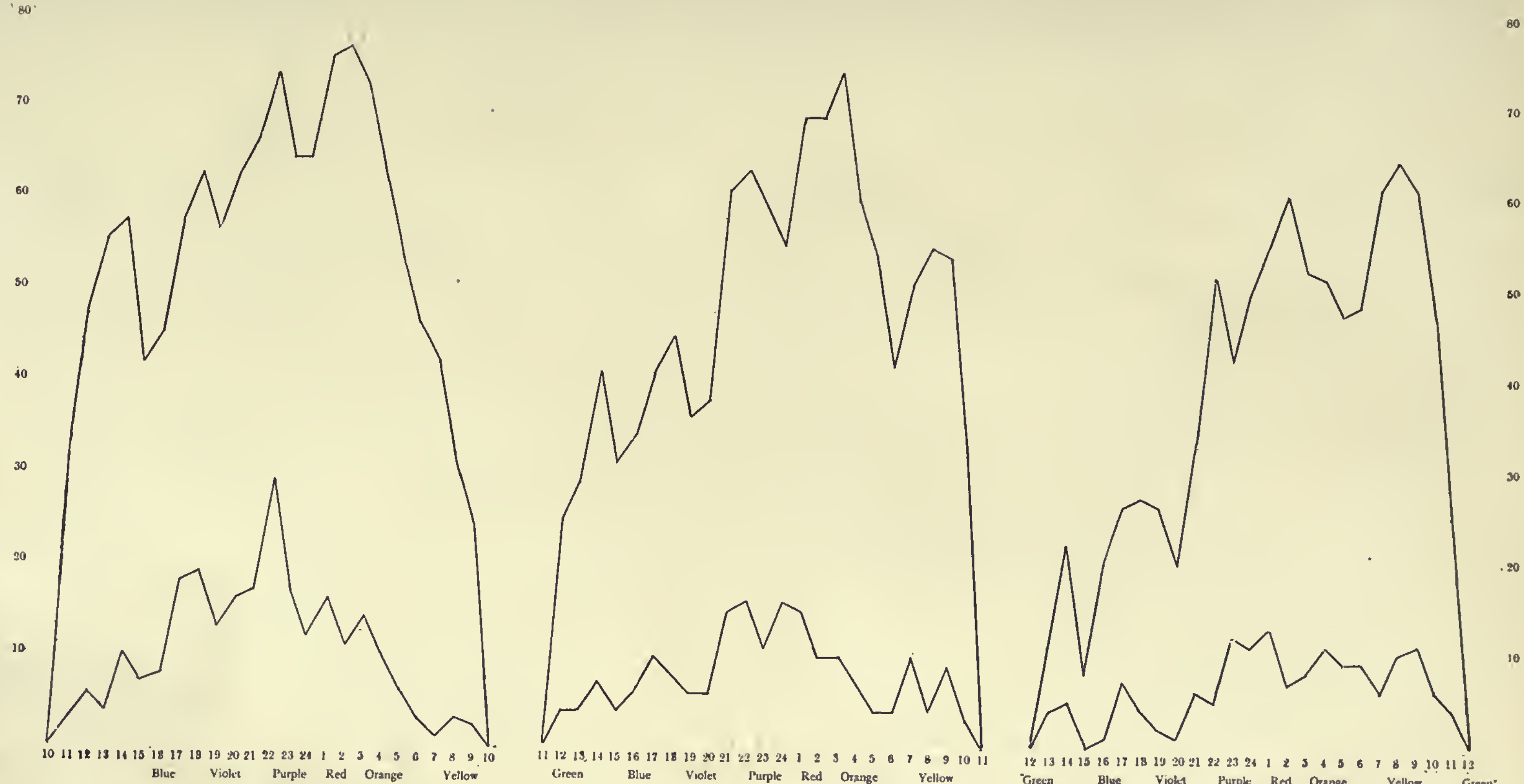
VII
YELLOW ORANGE

VIII
ORANGE YELLOW



IX
YELLOW

Plate 3

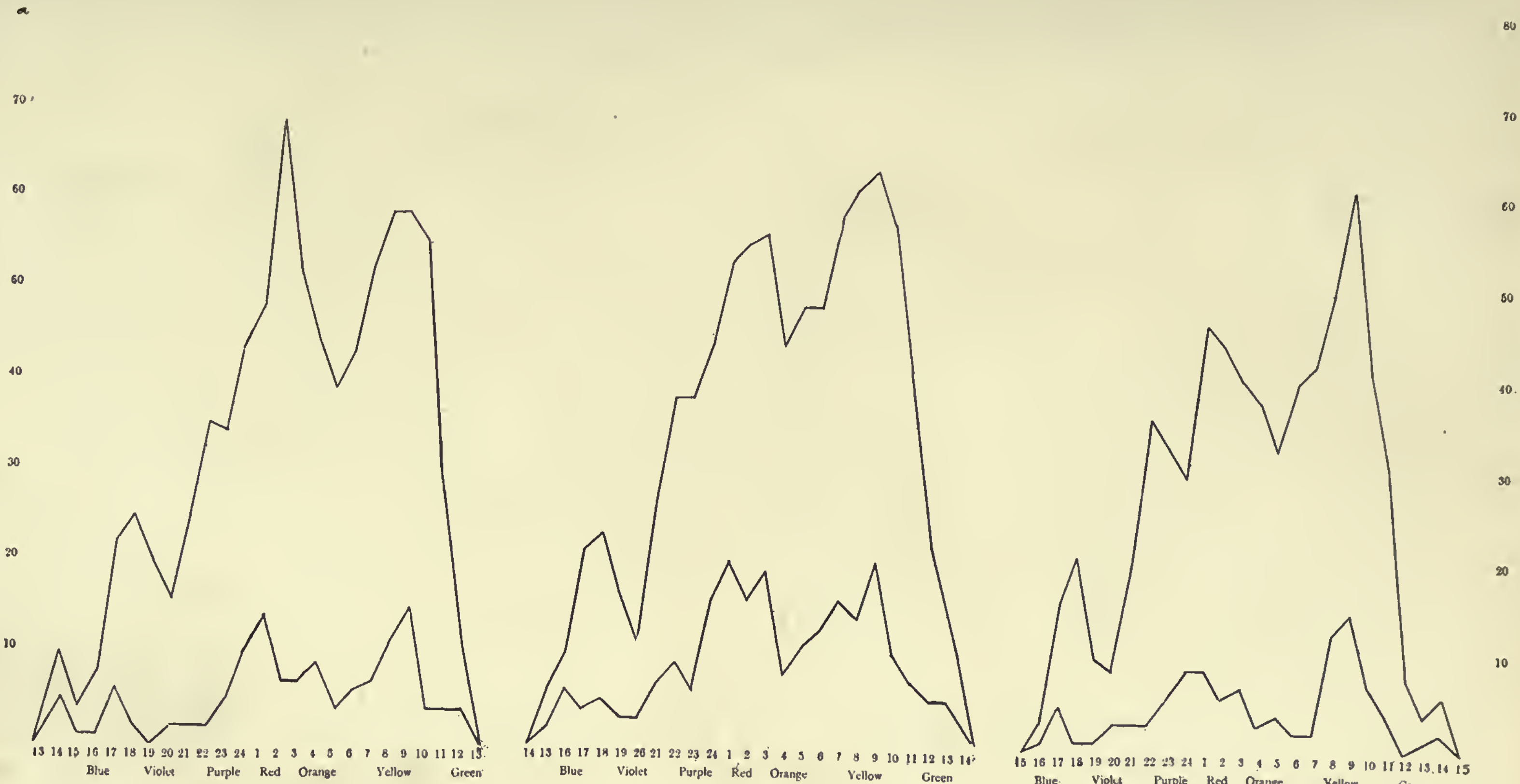


X
GREEN YELLOW

XI
YELLOW GREEN

XII
YELLOWISH GREEN

Plate 4



XIII
GREEN

XIV
BLUE GREEN

XV
GREEN BLUE

Plate 5

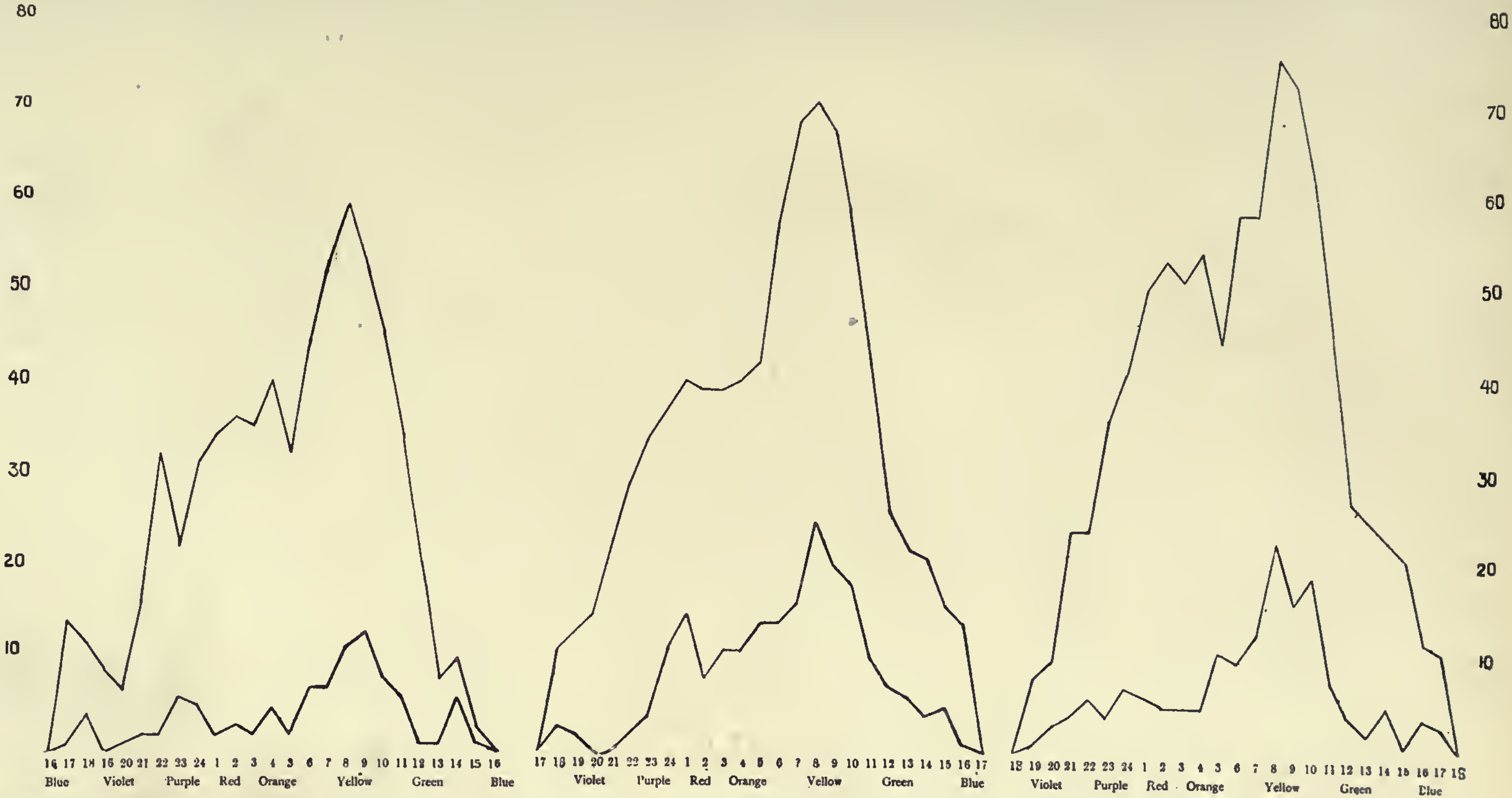


Plate 6

XVI
BLUE

XVII
VIOLETISH BLUE

XVIII
VIOLET BLUE

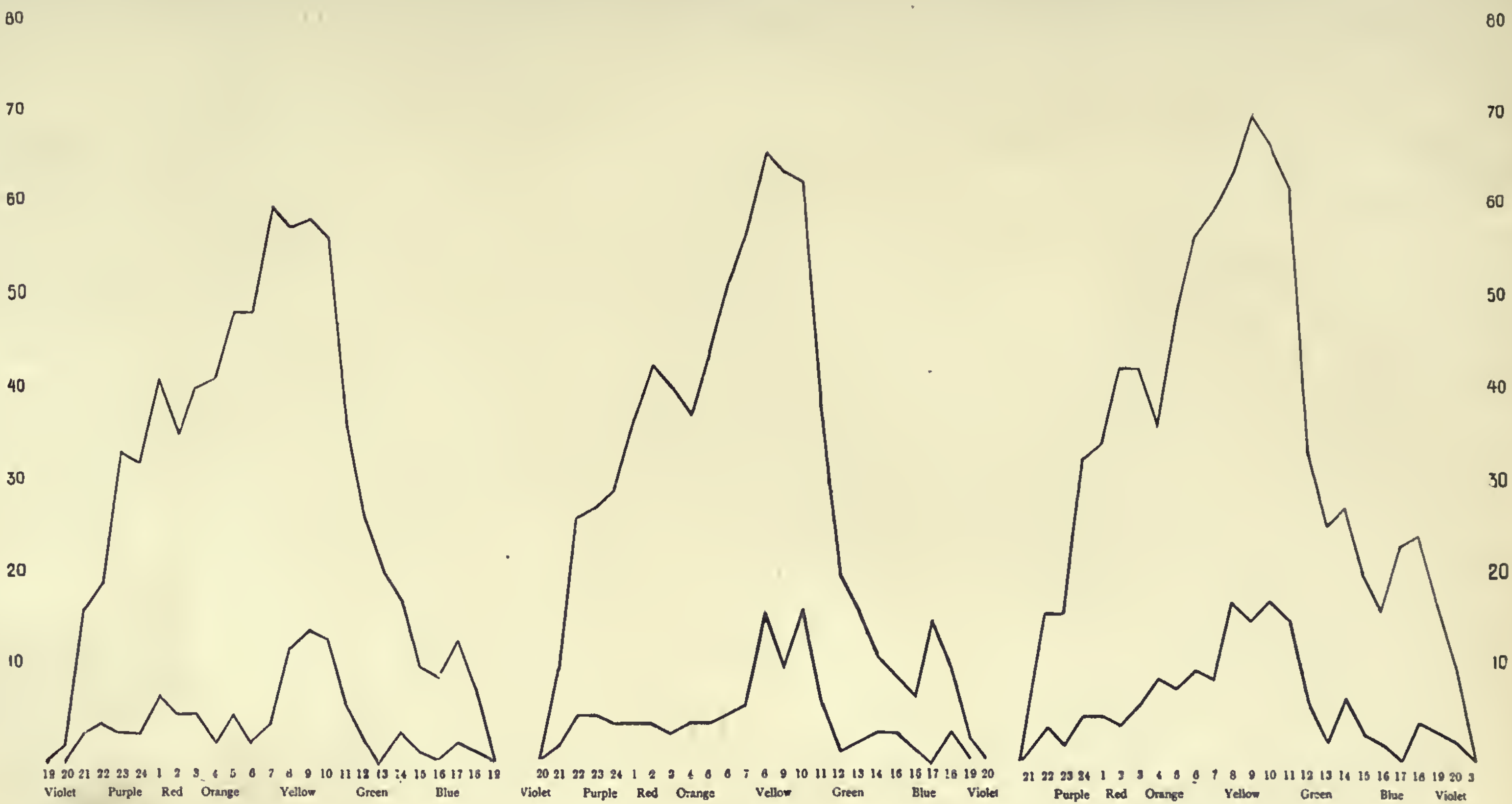


Plate 7

XIX
BLUE VIOLET

XX
VIOLET

XXI
PURPLISH VIOLET

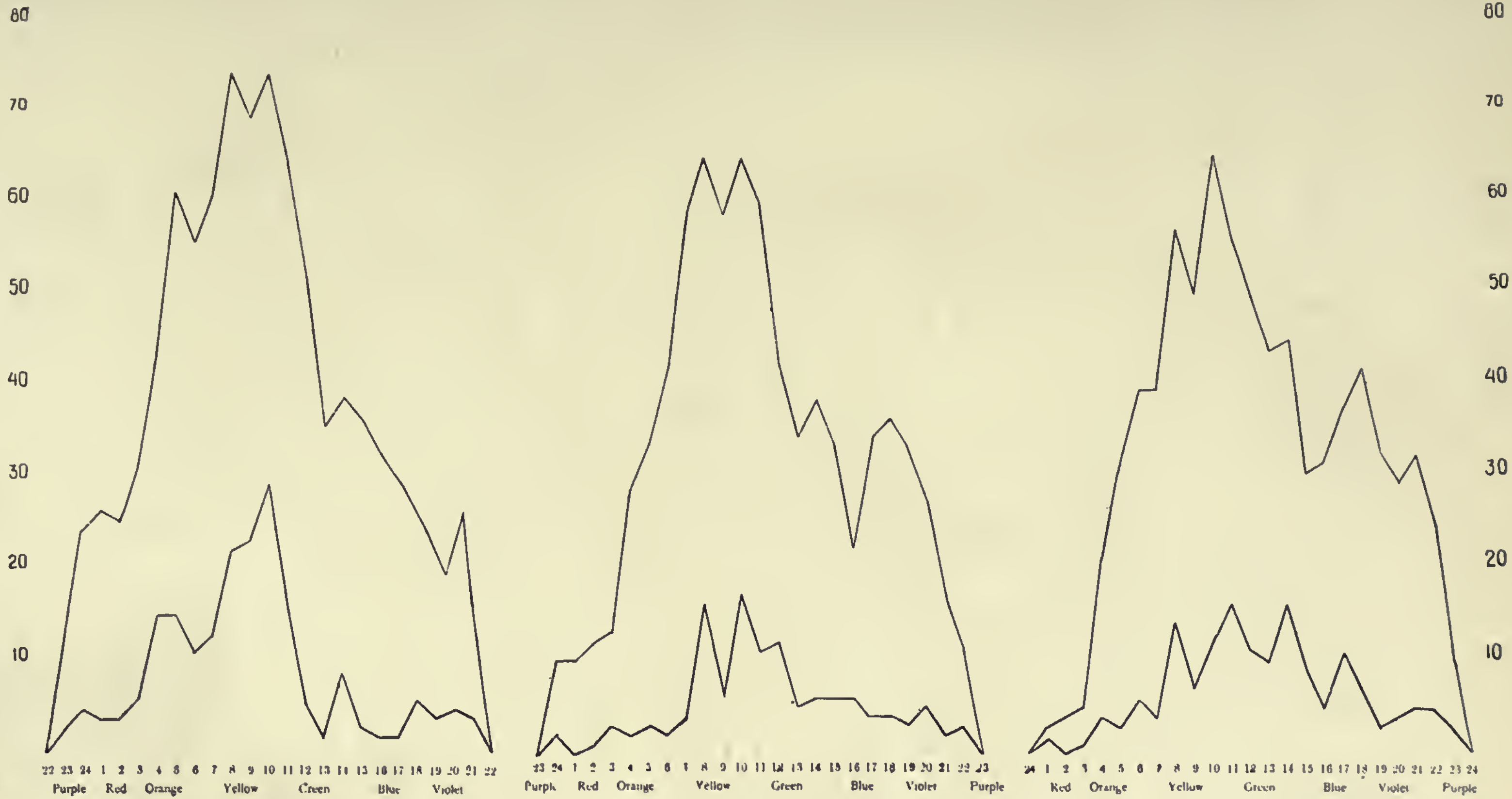


Plate 8

XXII
PURPLE VIOLET

XXIII
VIOLETISH PURPLE

XXIV
PURPLE

A glance through the twenty-four curves will immediately reveal a vast difference in the behaviour of the different colours. While some of the curves are sharply peaked and reach to a great height, others, although covering a greater surface, are flattened at the top and keep at a moderate height. Some are rugged while others have conspicuously rounded corners. Upon closer inspection we find that we can distinguish among them types characterized by distinctly different shapes.

Curves XXIV, I, II and III, representing the combinations with purple and red as 'combining colours,' have two outstanding maxima in the curves for the pleasant combinations, the major of which is at 10-11 (greenish-yellow), the minor at 18 (violet-blue). The maximum of the curve representing the most pleasant combinations, which is at 14 (blue-green), does not coincide with that of the curve above. There are, however, two minor maxima constant in the four curves, at 10 and at 17 (violetish-blue), and there is also a minimum in the lower curve at 16 which throughout coincides with the decided minimum in the upper curve. Curve XXIV leads now to the type to which XXIII belongs in showing the yellow-green already as prominent as blue-green, and the fourth maximum already makes its appearance.

Curves IV to X, in which the 'combining colour' is yellow or in the vicinity of yellow, show distinctly different features although Curve IV forms the transition between this and the first type. Instead of one decided maximum in the curve of pleasantness we have a number of maxima of almost equal height. The curves at the ends of the type are, to some extent, an exception, Curve IV shewing still great similarity to the red type, while Curve X has already much of the characteristic of the green type. The curves for the most pleasant combinations shew similar features in IV, V, VI, and VII, where there are no decided maxima and the curve is not high generally. It becomes higher in VIII, IX and X, and in X it has already assumed its decidedly peak-shaped form. The principal maximum gradually shifts from 22 in IV to 14 in VII, and then goes back to 22 in X.

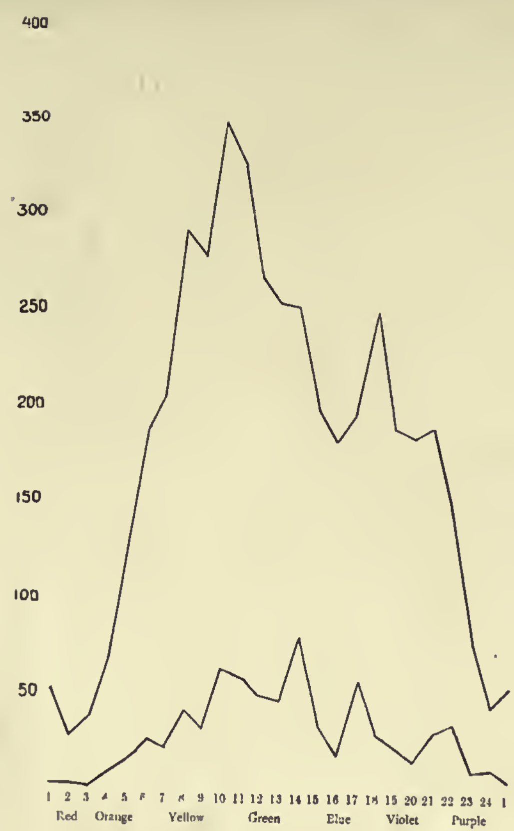
Curve X leads over to the next type consisting of the curves from XI to XIV. In the colours which these curves represent green predominates. In these also the maxima of the pleasant combinations are not very decided, but more decided than in the former group. In Curve XI the maximum is at 3, the yellow 7-9 showing a marked tendency to rise. In the following Curves XII and XIII, the yellow is struggling for supremacy and in XIV and XV it has decidedly attained it.

The regularity of the gradual changes can be seen very plainly by tracing the course of one of the maxima or minima, *e.g.*, the colour 18, representing one of the minor maxima. In Curve X its ordinate is 62, in XI it has gone down to 45, in XII it is 27, in XIII 25, in XIV 23, and in XV 21. Its continued course downward is still apparent in the first curve of the next type, where it has gone down to 13. Let us take also the minor maximum found at 14. In Curve IX the value of its ordinate is 63, in Curve X, 57; in Curve XI, 41; in Curve XII, 22; in Curve XIII, 10; and in Curve XIV it is of course zero.

In the curve for the most pleasant combination, the decided maximum at the purple of Curve X is going down, flattening out and leaning towards the red in XI, and in Curve XII, struggling with another maximum that is rising at 9 (yellow). In Curves XIII and XIV, 1 and 9 are very equally balanced, but in XV 9 has decidedly got the supremacy.

The next type includes the curves from XV (which makes the transition) to XVIII. The abrupt rise noticeable in the pleasant combination curves of the first type we find repeated in these, but it is now at the right. The supremacy attained by the colour 8 in the last type is retained throughout this group. In XV we have still some minor maxima which are found, though less marked, in XVI; but the curve is beginning to take a less jagged appearance and in XVII it has rounded out until there is only one maximum.

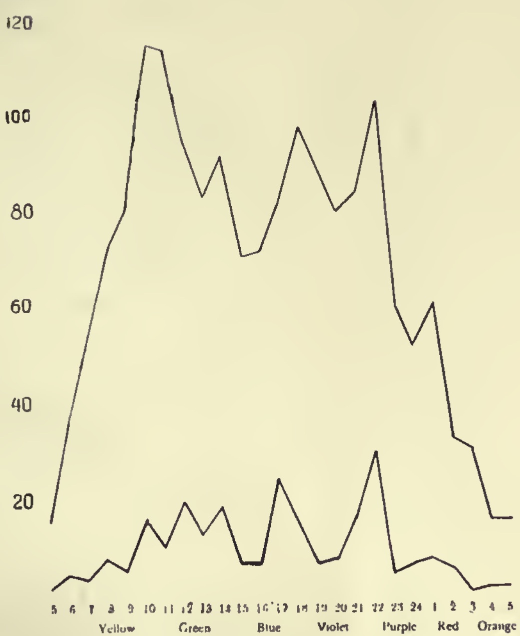
In XIII a gradually growing similarity is apparent between this curve and the one that represents the most pleasant combination, until in XV their maxima coincide. This coincidence of the maxima continues not only throughout this type but in the next also. In XV, for instance, the maximal points are at 9 in both, in XVI at 8 and 9 respectively, in XVII and XVIII at 8. In XIX there are two maxima in the 'pleasant' curve, 7 and 9, and the maximal point in the 'most pleasant' curve is at one of them. In XX the maximal point of the 'pleasant' is at 8, while the most pleasant has maximal points at 8 and 10. In XXI they are at 9 in the former and at 8 and 10 in the latter. In XXII we find the maximal points coinciding at 10, there being a second maximal point at 8 in the 'pleasant' curve, and in XXIII they perfectly



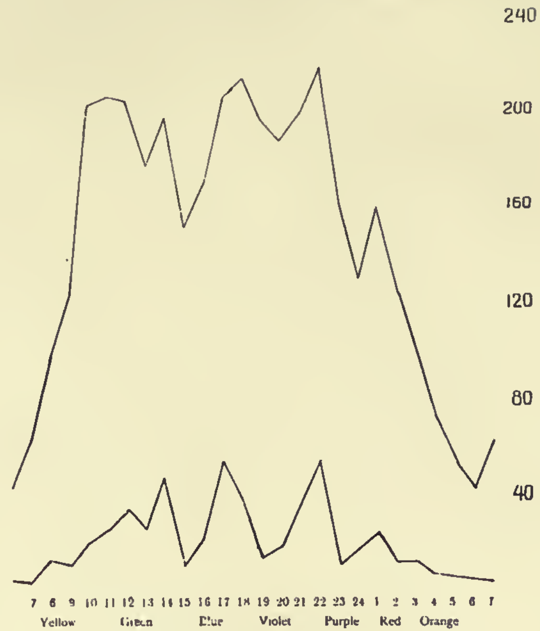
XXV (24, 1, 2, 3 and 4)
RED

Plate 9

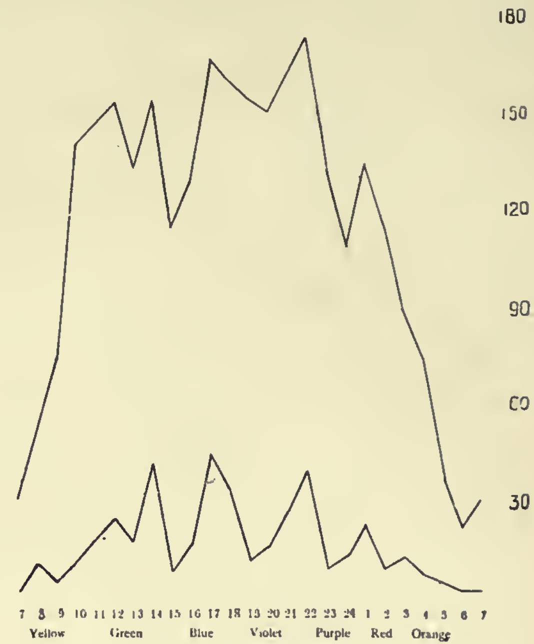
XXV (1, 2 and 3)
RED



XXVII (4 and 5)
RED ORANGE

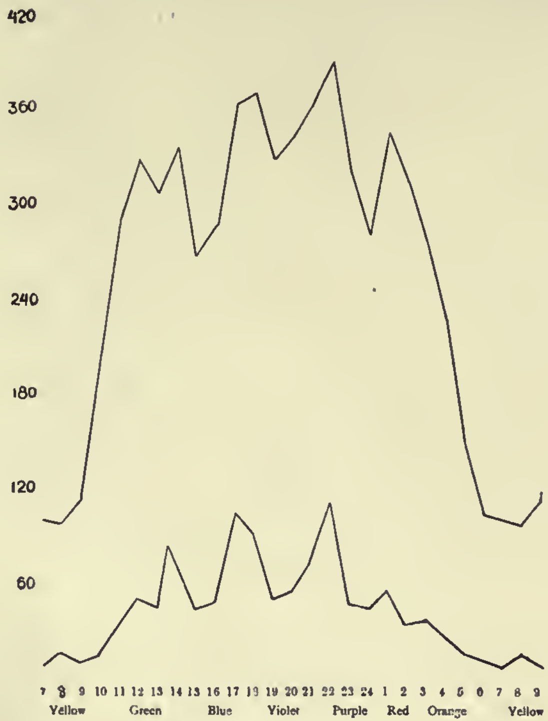


XXVIII (4, 5, 6 and 7)
ORANGE

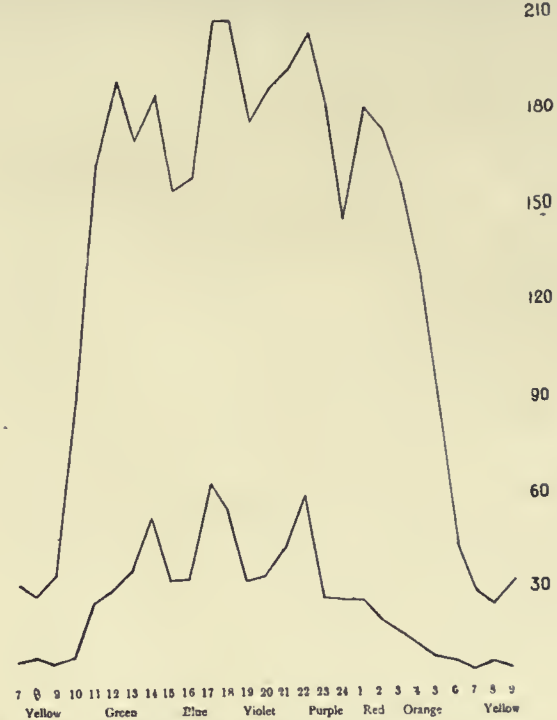


XXIX (5, 6 and 7)
YELLOW ORANGE

Plate 10

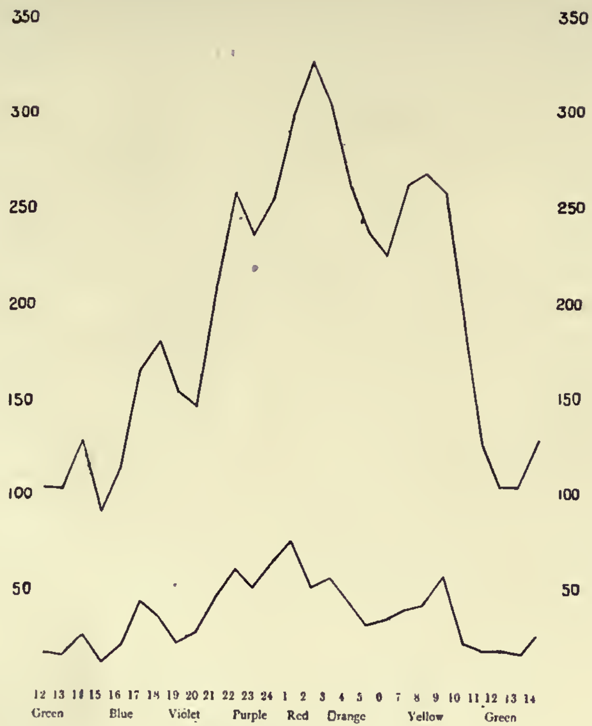


XXX (5, 6, 7, 8, 9 and 10)
YELLOW



XXXI (7, 8 and 9)
YELLOW

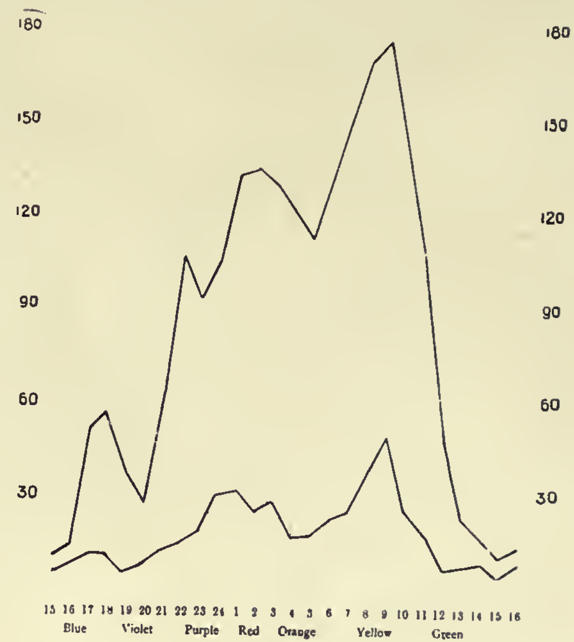
Plate 11



XXXII (10, 11, 12, 13 and 14)

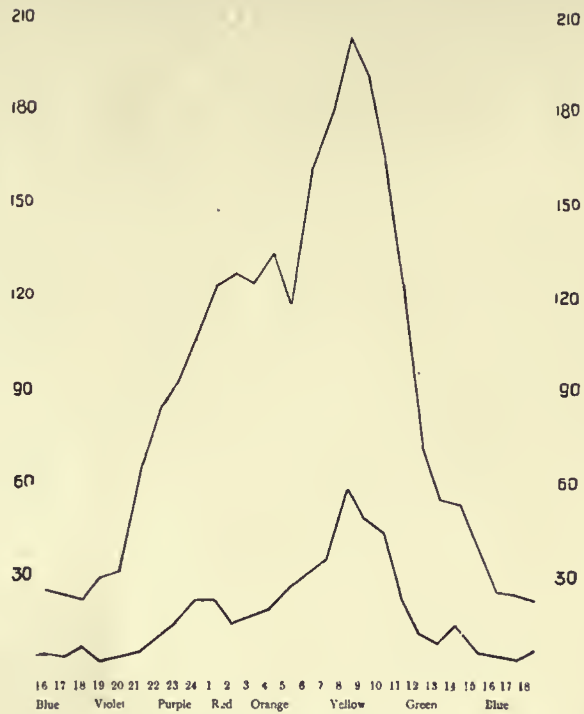
GREEN

Plate 12



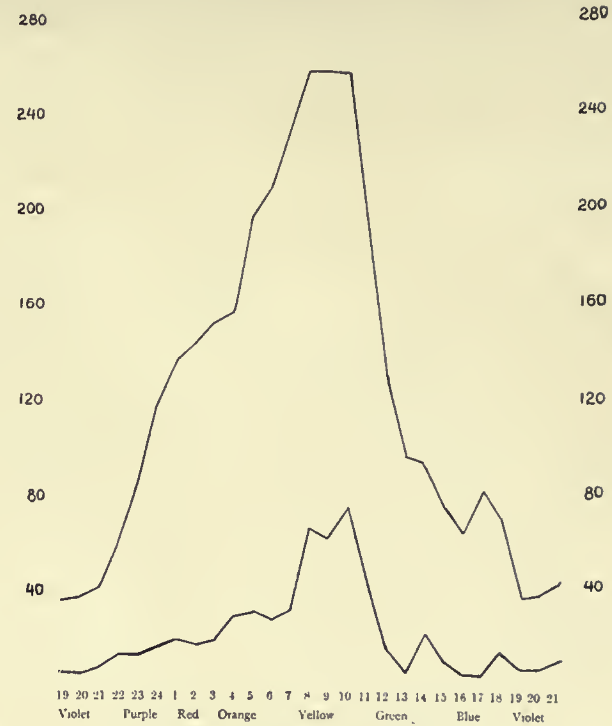
XXXIII (14, 15 and 16)

GREEN BLUE

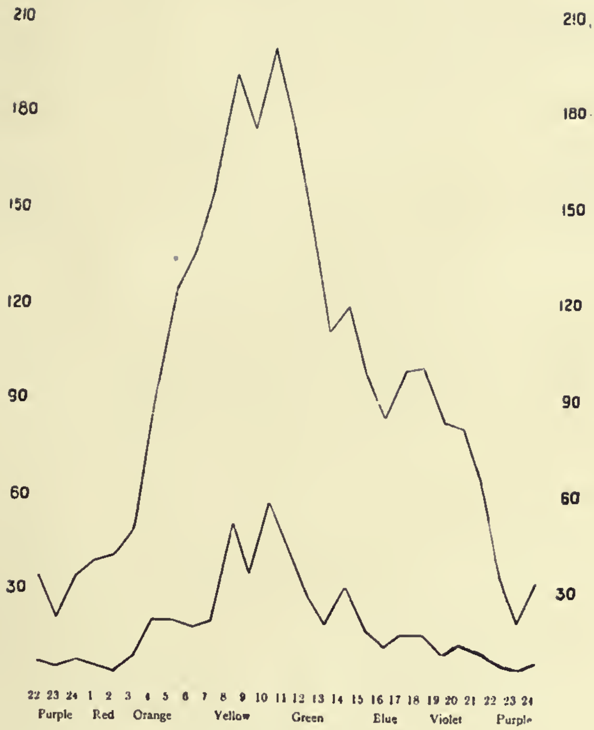


XXXIV (16, 17 and 18)
Blue

Plate 13



XXXV (19, 20, 21 and 22)
Violet



XXXVI (22, 23 and 24)

Plate 14 PURPLE

coincide, being at 8 and 10 in both. It will be noticed that this marked agreement in the two curves, both in the general contour and in the maximal points, but especially in the latter, is found in those curves representing the more refrangible end of the spectrum—that is with the greens, blues and violets.

The next group ranging from XIX to XXIII, upon which we have already touched in noticing the similarity between the upper and lower curves, includes the violets and the purples that are violetish. In the 'pleasant' curve of XIX already one of the characteristics of the next type—*i.e.*, of that first noticed—begins to appear. A small peak begins to arise from the right where before had been an abrupt fall. This occurs at 17-18, where in the first type is one of the more important of the minor maxima. In XIX it is not so high, but in XX to XXIII it becomes more decided. In XXI, 14, which forms a second minor maximum in the first type, begins to appear. Neither of these has, however, sufficiently asserted itself to permit of there being more than one decided maximum. The maximum which was at 8 in the preceding group now begins to shift to the right. Agreement between XXII and XXIII is noticeable in both, 8 and 10 are equally high, their ordinates being the same, with an almost equal drop between them. The gradual changes to which attention has been called in passing from type to type may also be seen in passing from curve to curve.

To sum up, it will be noticed as a general characteristic that the curves representing the colours that correspond to the ends of the spectrum and the intermediate colour, purple, are sharply pointed and high, rarely with an abrupt rise at the ends, and that their maximum plays between 8 in the blue and 11 in the red, whereas the curves representing orange and yellow—even to yellow-green—are of quite a different shape. These are not so high, are flattened at the top and rise abruptly at the ends. All the others, IV on the one hand and those from X to XIV on the other, form the transition. This, perhaps, will be better seen in Curves XXV to XXXVII, each of which represents a group of one series of colours, in combination. In order to facilitate comparison the curves are all drawn in the same *size* as the twenty-four original curves; the *ordinates*, consequently, have different values according to the number of curves which have been united in the combination. Each curve, therefore, has its scale marked at the side.

Curve XXV is a combination of XXIV, I, II, III and IV, and includes all the colours which come under the head of red. Naturally we should expect to find the same characteristics in these curves as in those of which they form the combination. But those characteristics are slightly modified; in XXV, for example, some of the minor maxima drop out, although the general characteristics remain the same. In order to convince the reader that the shape of the curve depends far less on the extension of the region which we include in our combination than on the position of it, we give in XXVI also a combination of 1, 2 and 3. The omission of the purple and orange extremes of the colours which could be termed red causes only extremely slight changes. The curves are almost identically the same.

In XXVII, XXVIII and XXIX, we see the same broad, plateau-like formation as in the curves of which they form the combination. Curve XXVIII is the representative for orange, whilst in the two other curves the slight changes caused by inclination towards the red or yellow side may be seen. We see the maximum point of the 'pleasant' curve travelling from 10 in the red-orange (with 22 as a rival), through the orange curve, in which the different maxima keep about on the same level, to 22 in the yellow-orange, which there has the undoubted supremacy. The decided minima in both the 'pleasant' and 'most pleasant' curves at 15-16 and 19-20 are worthy of note. It is interesting to notice that these minima can be traced far into the yellow and green; they are distinctly marked in the curves for green and blue-green, XXXII and XXXIII.

Curves XXX and XXXI show combinations of those colours which may be termed yellow. It will again be realized that the addition of the colours 5, 6 and 10, *i.e.*, the yellows bordering on orange and green respectively, does not change the general character of the curve. It is presumed that the reader will now have seen the significance of these facts, so that in the following combinations it is sufficient to give the curve for one extension only of the region.

Curve XXXII (green) is a combination of all the curves of the third type. We have here brought together all the changes in passing from the first curve which contains any green (yellow still predominating) to a decided blue-green. In this combined curve it will be noticed that the maximum is at 2 (red) with minor maxima at 22 and 8.

The gradual shifting of the abrupt rise from the right as found in the last type (XIX to XXIII) to the left as found in the first (XXIV to IV) can be seen very plainly from Curves XXXIII, XXXIV, XXXV, XXXVI and XXV. In XXXIV (blue) the abrupt rise is at the right, and the maximum at 8, in XXXV (violet) the jagged appearance is noticed on the right side, and in XXXVI (purple) the abrupt rise already referred to has shifted altogether to the left where it is found in XXV (red), while in all these the maximum remains constant.

The similarity between the 'pleasant' and 'most pleasant' curves, to which attention was called in the discussion of the individual representations, can perhaps be better traced in these combination curves. In XXV and XXVI (red) there is the least resemblance of any. In all the others the maximal and minimal points of both curves coincide to some extent and the resemblance in the general contour is in many cases striking.

Curve XXXVII represents a combination of all the curves from I to XXIV. As forming the most pleasant combinations with all the others, 8 (orange-yellow) was chosen most frequently (251 times), 10 (yellow-green) falling just below (245 times).[1] In the pleasant combination 8 falls a little below 10, the latter being chosen 1263 times out of a possible 1380, the former 1203 times.[2] In this combination curve we find an almost perfect coincidence between the curve for the pleasant and that for the most pleasant combinations. Both of these have, besides the absolute maximum in the yellow, relative maxima in the blue-green, the violetish-blue, and the violetish-purple, and decided minima in the orange, the green, the blue, the violet and the purple; those in the blue and violet being the lowest.

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[1] See Table V.

[2] See Table III.

TABLE VI

Pleasant Combinations

Combination of Colours.	Times Chosen.	Combination of Colours.	Times Chosen.	Combination of Colours.	Times Chosen.	Combination of Colours.	Times Chosen.
19—20	2	3—23	13	2— 5	24	19—24	32
1—24	3	5— 6		4— 6		21—24	
15—16		17—19		5— 7		4— 7	33
2—24	4	1— 3	14	6— 9		5— 9	
13—15		6— 8		9—10		5—15	
3—24	5	8— 9		18—21		5—23	
6— 7	6	16—17		18—22		12—21	
14—15		17—20	15	22—24		15—23	
2— 3	7	4— 5	16	2—22	25	19—23	
16—20		13—20		11—12		1—16	34
1— 2	8	15—17		13—18		1—21	
12—15		16—21		13—21		11—16	
13—16		19—21		1— 4	26	13—23	
18—19		21 22		1—22		17—23	
2— 4	9	21—23		12—17		1— 5	35
15—20		14—19	17	12—19		2—19	
16—19		7— 9	18	20—22		3—16	
1—23	10	3— 5	19	5— 8	27	13—22	
13—14		19—22		12—18		1—20	36
14—16		12—16	20	14—21		2—16	
15—19		12—20		20—23		4—21	
18—20		13—19		4—23	28	11—19	
20—21		15—21		3— 6	29	15—22	
23—24		4—24	21	11—13		18—23	
7— 8	11	14—17		17—22		4—20	37
12—13		12—14	22	20—24		17—24	
14—20		13—17		15—24	30	4—15	38
17—18		16—23		3—22	31	11—20	
22—23		14—18	23	5—24		14—22	
2—23	12	17—21		8—10		14—23	
3— 4				11—15		2—17	39
16—18				16—24		3—17	
				5—16	32	5—13	
				10—11		6—24	
				16—22		7—24	

TABLE VI

Pleasant Combinations—*Continued*

Combination of Colours.	Times Chosen.	Combination of Colours.	Times Chosen.	Combination of Colours.	Times Chosen.	Combination of Colours.	Times Chosen.
1—17, 3—7, 3—15, 3—19, 3—20, 4—16, 4—17, 6—15	40	1—15, 6—10	46	2—14, 6—22, 8—11, 10—13, 11—24	55	2—8, 4—10, 10—18, 10—20	62
1—19, 2—6, 4—19, 7—15, 11—14, 11—17, 18—24	41	5—12, 10—12	47	3—14, 6—21, 8—24, 10—19	56	1—9, 8—21, 9—14, 9—20, 11—22	63
2—20, 2—21, 3—21, 5—17, 6—11, 6—23, 7—10, 10—15, 12—23	42	1—13, 4—9, 5—14, 5—19, 5—21, 6—12, 6—14, 6—19	48	3—9, 6—17, 7—20, 8—19, 10—14, 10—17	57	8—12, 8—23, 10—23, 10—24	64
4—22, 5—20, 6—13, 13—24	43	9—24, 12—24	49	3—8, 6—18, 7—14, 7—18, 7—23, 8—13, 9—13, 9—19, 9—23	58	8—20	65
2—15, 4—13, 4—14, 5—18, 14—24	44	1—18, 2—7, 8—15	50	7—19, 7—21, 8—16, 11—23	59	10—21	66
1—7, 4—8, 6—16, 10—16, 11—18	45	3—13, 3—18, 4—12, 6—20, 7—11, 12—22	51	2—9, 2—12, 4—11, 5—22, 7—22	60	9—17	67
		3—12, 7—13	52	7—12, 8—14, 9—12, 9—15, 11—21	61	2—13, 7—17, 9—22	68
		1—11, 2—18, 5—10, 7—16, 9—16	53			1—11, 2—11, 9—21	69
		1—6, 4—18, 5—11, 9—11	54			8—17	70
						1—8	71
						3—10, 9—18	72
						8—22, 10—22	73
						3—11	74
						1—10, 8—18	75
						2—10	76

TABLE VII

Most Pleasant Combinations

Combination of Colours.	Times Selected.	Combination of Colours.	Times Selected.	Combination of Colours.	Times Selected.	Combination of Colours.	Times Selected.
1—2		4—7		6—10		10—13	
1—3		4—19		7—8		11—12	
1—23		4—23		8—9		11—13	
2—3		5—6		8—10		11—15	
2—24		5—7		10—11		12—13	
3—4		5—16		14—19		12—18	
5—9	0	6—8		14—20		14—17	4
6—7		6—15		15—20		16—18	
12—15		6—19		15—21	3	17—23	
13—19		6—23		15—22		18—21	
16—19		7—15		17—18		18—23	
17—20		9—10	2	18—20		19—22	
17—21		12—19		19—21		20—24	
19—20		13—18		19—23		21—22	
		13—20		19—24			
2—23		13—21		22—23			
3—5		13—22		23—24		1—21	
3—24		14—15				2—18	
7—9		16—21		1—20		2—19	
7—10		16—22		1—22		3—18	
12—16		17—19		2—7		3—19	
12—20		17—22		2—20		4—16	
13—15	1	20—21		2—21		4—18	
13—16				2—22		5—8	
15—16				3—7		5—19	
15—18				4—20		6—20	
15—19		2—4		4—24		12—14	5
16—17		2—5		5—11	4	12—22	
16—20		2—16		5—13		13—14	
18—19		3—9		5—15		13—23	
		3—20		5—20		14—18	
1—4		3—23	3	6—9		15—17	
1—16		4—6		6—11		16—24	
1—24	2	4—8		7—19		20—22	
2—6		4—15		7—23		20—23	
3—16		5—23		7—24		21—24	
4—5		5—24		8—11		22—24	

TABLE VII

Most Pleasant Combinations—*Continued*

Combination of Colours.	Times Selected.	Combination of Colours.	Times Selected.	Combination of Colours.	Times Selected.	Combination of Colours.	Times Selected.
1— 5	6	11—14	7	2—10	11	9—13	15
1—18		12—17		3—17		9—15	
2— 8		14—21		4—12		9—21	
2—15		18—24		4—17		11—21	
3—21				5—14			
3—22		1— 6	8	5—18		1—10	16
4— 9		1— 7		6—22		2—14	
5—10		2—17		8—13		7—14	
6—13		3— 8		8—16		7—17	
6—24		3—12		9—12		8—20	
7—12		4—14		11—23		8—23	
7—20		5—21		12—24		9—18	
9—23		10—16		17—24		10—20	
10—12		11—18				11—22	
11—16				1—12	12	11—24	
11—19		1—15	9	8—19		14—24	
11—20		2— 9		10—24			
12—21		4—13		12—23		8—21	17
13—17		4—21				10—21	
14—16		5—12		6—14	13	10—23	
14—23		6—12		7—18			
15—23		7—21		7—22		10—17	18
16—23		9—11		8—15			
18—22		14—22		9—16		3—14	19
		15—24		10—19		10—18	
1— 9	7						
1—19		1— 8	10	1—13	14	1—14	20
2—12		2—11		3—10		9—14	
2—13		3—11		5—17		9—17	
3— 6		4—10		6—17			
3—13		6—18		8—14		8—22	22
3—15		6—21		8—24			
4—11		7—11		9—19		8—18	23
6—16		8—12				9—22	
7—13		9—20		1—11	15		
7—16		10—14		1—17		8—17	25
9—24		11—17		4—22			
10—15		13—24		5—22		10—22	29

TABLE VIII

MAXIMA OF THE PLEASANT COMBINATIONS

COLOUR combined with	COLOUR.	NUMBER OF TIMES CHOSEN.
Red (2)	Green yellow (10)	76
Purplish red (1)	Green yellow (10)	75
Orange yellow (8)	Violet blue (18)	75
Orange red (3)	Yellow green (11)	74
Orange yellow (8)	Purple violet (22)	73
Green yellow (10)	Purple violet (22)	73
Yellow (9)	Violet blue (18)	72
Orange yellow (8)	Violetish blue (17)	70
Yellow (9)	Purplish violet (21)	69
Yellow orange (7)	Violetish blue (17)	68
Red (2)	Green (13)	68
Orange yellow (8)	Violet (20)	65
Orange yellow (8)	Yellowish green (12)	64
Orange yellow (8)	Violetish purple (23)	64
Green yellow (10)	Violetish purple (23)	64
Green yellow (10)	Purple (24)	64
Yellow (9)	Blue green (14)	63
Reddish orange (4)	Green yellow (10)	62
Yellow (9)	Green blue (15)	61
Orange (5)	Purple violet (22)	60
Orange yellow (8)	Blue (16)	59
Yellow orange (7)	Blue violet (19)	59
Yellowish orange (6)	Violet blue (18)	58

TABLE IX

Maxima of the "Most Pleasant" Combinations

Colour combined with	Colour.	Number of Times Chosen.
Green yellow (10)	Purple violet (22)	29
Orange yellow (8)	Violetish blue (17)	25
Yellow (9)	Purple violet (22)	23
Orange yellow (8)	Violet blue (18)	23
Purplish red (1)	Blue green (14)	20
Yellow (9)	Blue green (14)	20
Orange red (3)	Blue green (14)	19
Orange yellow (8)	Purplish violet (21)	17
Green yellow (10)	Purplish violet (21)	17
Green yellow (10)	Violetish purple (23)	17
Red (2)	Blue green (14)	16
Yellow orange (7)	Blue green (14)	16
Yellow orange (7)	Violetish blue (17)	16
Yellow green (11)	Purple violet (22)	16
Yellow green (11)	Purple (24)	16
Orange yellow (8)	Violet (20)	16
Green yellow (10)	Violet (20)	16
Blue green (14)	Purple (24)	16
Reddish orange (4)	Purple violet (22)	15
Orange (5)	Purple violet (22)	15
Yellow (9)	Green (13)	15
Yellow (9)	Green blue (15)	15
Yellowish orange (6)	Violetish blue (17)	14
Yellow (9)	Blue violet (19)	14
Yellow (9)	Blue (16)	13
Purplish red (1)	Yellowish green (12)	12
Yellowish green (12)	Violetish purple (23)	12

This total combination Curve (XXXVII) gives a representation of the frequency with which the individual colours are chosen in combinations. It does not directly reveal the frequency of the combinations themselves, which, however, can be seen from the following tables. In Table VI the first column gives the combinations of colours designated by their numbers (the names of these can be easily ascertained from Table I); the second column shows how often the combination was selected as agreeable. Table VII in an analogous manner represents the frequency of the selection as a most agreeable combination.[1] In Tables VIII and IX a further synopsis is given of the frequency of these combinations which figure as maxima in the curves. In these cases the word-designations of the colours have been added. It will be seen from these tables that the combination of red and reddish-purple respectively, with green-yellow, and the combinations between the different oranges and violet or violetish-blue, and those between the yellows and purples, stand foremost in the scale of frequency as pleasant combinations. Among the others, green proper (without a tendency to yellow) and blue proper (without a tendency to violet) are seldom chosen. It is always the yellowish-green and the blue tending toward red, which have the preference. This indicates clearly the inclination of the æsthetic centre of gravity towards the 'warm' side of the manifoldness of colour as already noted above. It remains to be investigated whether this might not be partly due to the defects in distribution of qualities in the Prang colour system, which is less differentiated in the 'cold' than in the 'warmer' part of the manifoldness. It might be said that the observers preferred the 'warmer' combinations because there were more of them; but, on the other hand, one could just as well claim that the colder combinations would be preferred because, owing to their comparatively small number, they were more conspicuous. We think, after all, that this circumstance did not influence the observers.

The attention of those who would expect a sharper separation of pleasant and unpleasant combinations is called to the following two points: (1) Pleasant and unpleasant are always relative terms, and therefore there is no possibility of a sharp line of demarcation; (2) The colours which we used were highly saturated colours, and the combinations of such never reach that degree of agreeableness

[1] Tables VI and VII are constructed from Tables III and V respectively.

which is possible in combinations in which the components not only differ in quality but in saturation also. On the other hand, the full saturation of the individual components is in itself always something agreeable and thus prevents the combinations from dropping very much below the point of indifference. Indeed our observers, when seeing the twenty-four colours arranged on the black surface, repeatedly remarked that the beauty of the individual colours was such that it was difficult to find combinations which were not agreeable.

The next point to which attention is called is the bearing of the foregoing results upon the theories of colour æsthetics that have been hitherto advanced. It has already been noticed that the majority of writers on colour æsthetics hold that the most pleasant combinations are formed by complementaries. The predominance of the view mentioned without the support of experimental investigation makes the results of any experiments which really deal with the question of peculiar interest. Cohn, apparently having this theory in mind, chose his colours with special regard to the complementary relation, and partly for that reason, but more largely because he used so few colours his results (as already indicated) are far from being conclusive in proving, as he thinks they do, that complementary colours form the most agreeable combinations. With only ten colours of the colour-circle, when an observer is asked to choose the most pleasant combination with any colour he is compelled to choose the complementary of that colour or one at a considerable distance from it, whereas if there were intermediate qualities he might choose one of them. In our own experiments with twenty-four colours a decided preference for the intermediate qualities was found to be actually manifested.

It is necessary to emphasize the fact that during the course of our experiments we did not know which of the colours used were complementary. This excluded any possibility of the observers being influenced by knowledge regarding complementary relations. It was only after the experiments were completed that careful trials were made in order to determine the mutual relations of the colours of the Prang system. Discs were constructed of the pigment colours used in the experiments, and colour equations were made. These discs were 153 mm. in diameter and the equations were made by means of black and white discs 80 mm. in diameter, placed with them on the rotation apparatus. The results of the experiments are collated in Table X. The first column contains the number of degrees of the colours of our series from 1 to 24; the second

TABLE X

Colour Equations

			Complementary.					Intensity.			
Degrees.	Colour.		Degrees.	Colour.	Degrees.	Colour.		Degrees.		Degrees.	
111	of 1	+	[103	of 14 +	146	of 15]	=	328	of black +	32	of white.
81	of 2	+	[103	of 14 +	182	of 15]	=	319	" +	41	"
72	of 3	+	[105	of 14 +	183	of 15]	=	315	" +	45	"
62½	of 4	+	297½	of 15			=	313	" +	47	"
66½	of 5	+	[152½	of 15 +	41	of 16]	=	305	" +	55	"
63	of 6	+	[213	of 15 +	84	of 16]	=	303	" +	57	"
79	of 7	+	[148½	of 15 +	132½	of 16]	=	288	" +	72	"
94½	of 8	+	[23½	of 15 +	242	of 16]	=	274	" +	86	"
128	of 9	+	[115	of 16 +	117	of 17]	=	240	" +	120	"
83	of 10	+	277	of 21			=	289	" +	71	"
105	of 11	+	[33	of 21 +	222	of 22]	=	295	" +	45	"
132	of 12	+	[101	of 22 +	127	of 23]	=	302	" +	58	"
171	of 13	+	[57	of 22 +	132	of 23]	=	310	" +	50	"
235	of 14	+	[45	of 23 +	80	of 24]	=	315	" +	45	"
294	of 15	+	66	of 4			=	307	" +	53	"
270	of 16	+	[72	of 8 +	18	of 9]	=	277	" +	83	"
192	of 17	+	[138	of 9 +	40	of 10]	=	233	" +	127	"
218	of 18	+	[100	of 9 +	42	of 10]	=	140	" +	120	"
257	of 19	+	[70	of 9 +	33	of 10]	=	273	" +	87	"
283	of 20	+	[37	of 9 +	40	of 10]	=	296	" +	64	"
272	of 21	+	88	of 10			=	286	" +	74	"
251	of 22	+	[62	of 11 +	47	of 12]	=	295	" +	65	"
172	of 23	+	[138	of 13 +	50	of 14]	=	321	" +	39	"
107	of 24	×	[246	of 14 +	7	of 15]	=	315	" +	45	"

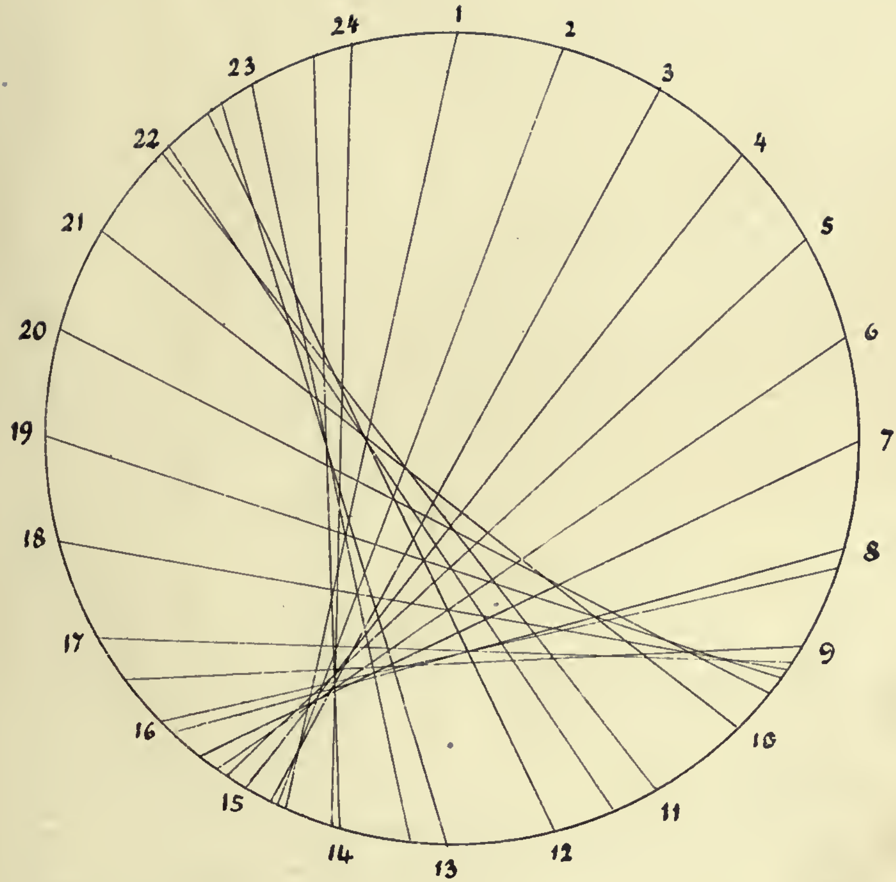

Plate 16

column shows the colour or combination of colours, and their quantities, necessary to produce, when mixed with the colour of the first column, colourless light; the third column indicates the light-intensity of the combinations. Each horizontal line forms an equation between the gray resulting from the mixture of the colours in question and that produced by black and white.

The accompanying figure represents graphically the complementary relations of the Prang standard colours. The twenty-four colours are arranged at equal intervals on the periphery of the circle. Each one is connected by a straight line with that part of the periphery at which — according to a calculation based on Table X — the complementary colour is situated. From this figure it is obvious that the differentiation in quality in the Prang system is by no means equally distributed, for the complementaries form clusters around 9, 15 and 23, and none of the connecting lines pass through the centre. This is certainly an imperfection of the Prang colour system. Had the Milton Bradley colours been used this defect would have been still greater, for in these the number of individual qualities is considerably smaller, and the breaks in the transition from green to blue and from violet to red are even wider. The fault lies in the fact that, in making up such systems of pigment colours, the dispersion spectrum, in which the differentiation of qualities is reduced to the smallest space in the yellow and blue-green, has always been copied. For purposes of psychological experimentation a series of pigment colours should be constructed without regard to the solar spectrum, and simply with reference to a colour circle with equally distributed differentiation.

In considering the relation in which the results of our experiments stand to the theory that the most agreeable combinations are those of complementary colours, we may group these results into four broadly marked classes: (1) those in which a maximum of agreeableness occurs at (or extremely near) the complementary, (2) those in which the most agreeable combinations occur near the complementary, but in which the complementary itself stands rather low in the scale of agreeableness, (3) those in which decided maxima are on both sides of the complementary, whilst the complementary itself is low, and (4) those in which the most agreeable combination does not occur at or near the complementary.

The curves of the first class of results (*i.e.*, those in which the most agreeable combinations occur at the complementary), are XI, XIX, XXI, and XXIV. The complementary of XI lies between 21 and 22, very near 22, while the most agreeable combinations occur at 22 and 24. The complementary of XIX lies between 9 and 10, closer to 9 than to 10, and the most pleasant combination occurs at 9, 10 having received but one judgment less. The complementary of XXI is 10, and the most agreeable combinations are found at 8 and 10. The complementary of XXIV is between 14 and 15, practically at 14, and the maximal points occur at 11 and 14. In the case of those four colours, then, it may be granted that the most pleasant combinations have occurred at (or exceedingly near) the complementary; although in three cases out of the four another colour near the complementary, and separated from it by a marked depression, was chosen an equal number of times.

The cases that fall into the second class (*i.e.*, those in which the most agreeable combination occurs near the complementary, the complementary itself being low) are enumerated in the following table:

TABLE XI

Colour.	Complementary.	Max. Point of Agreeableness and Number of Times Chosen.	Number of Times Complementary is Chosen.
1	Between 14 and 15, nearer 15	14, twenty times.	15, nine times.*
2	" 14 and 15, " 15	14, sixteen "	15, six "
3	" 14 and 15, " 15	14, nineteen "	15, seven "
8	" 15 & 16, very near 16	17, twenty-five "	16, eleven "
10	21	22, twenty-nine "	21, seventeen "
12	" 22 and 23,	23-1, twelve "	22, five " †
13	" 22 and 23, nearer 23	1, fourteen "	23, five "
14	" 23 and 24, " 24	1, twenty "	{ 23, six " { 24, sixteen "
16	" 8 and 9, " 8	9, thirteen "	8, eleven "
17	" 9 and 10, " 9	8, twenty-five "	9, twenty-three "
18	" 9 and 10, " 9	8, twenty-three "	9, sixteen "
22	" 11 and 12, " 11	10, twenty-nine "	11, sixteen "
23	" 13 and 14, " 13	10, seventeen "	13, five "

* When the complementary lies between two colours, as it does in most cases, we have taken the colour to which it is nearest.

† The complementary is here really midway between 22 and 23, slightly inclining to 23.

The curves of the third class (*i.e.*, those in which the decided maxima are on both sides of the complementary, but in which the complementary is low), are IV, V, VI, VII and XX. The complementary of IV is 15, and this is chosen but three times as the most pleasant combination, whereas 14 and 17 are chosen respectively eight and eleven times. The complementary of V, which is between 15 and 16, nearer to 15, lies again in a marked depression between the decided maxima at 14 and 17, which are chosen eleven and fourteen times respectively. The complementary of VI is between 15 and 16, much nearer 15. The latter colour was chosen but twice, while 14 and 17 were chosen thirteen and fourteen times respectively. The complementary of VII is between 15 and 16, slightly nearer to 15, which was chosen twice, while 14 and 17 were each chosen sixteen times. The complementary of XX is midway between 9 and 10; 9 was chosen ten times, 10 sixteen times, and the maxima occurred at 8 and 10, each being chosen sixteen times.

The cases which illustrate the fourth class (*i.e.*, those in which the maximum of the curve for the most pleasant combination does not occur at or near the complementary) are given in the following table:

TABLE XII

Colour.	Complementary and Number of Times Chosen.	Maximal Point and Number of Times Chosen.
4	15, three times.	22, fifteen times.
9	16, thirteen " 17, twenty "	22, twenty-three "
12[1]	22, five " 23, twelve "	4, eleven " 9, eleven "
14[1]	23, six " 24, sixteen "	9, twenty "
15	4, three "	9, fifteen "
23[1]	13, five " 14, six "	8, sixteen " 10, seventeen "

The fourth class comprises cases in which the most agreeable combinations are found at medium intervals between the complementaries, and therefore this class proves conclusively (without the support to be adduced from the other classes) that the most agreeable combinations *need not* occur either at or near the complementary, while classes (1), (2) and (3) shew that they may do so. Taking all the colours used in our experiments (as already noted some colours occur in two classes) we get the following summary: In four colours the most agreeable combinations occur at or very near the complementary, though in no case is the complementary the complete and sole possessor of the maximum. In thirteen colours the complementary, though near, is decidedly lower than the maximal point of agreeableness. In five colours the most agreeable com-

[1]This colour occurred also in Class (2), but as there are maxima at medium intervals also, it may be included here.

binations occur on both sides of the complementary and near it, while the complementary itself is low in the scale of agreeableness. In six colours the most agreeable combinations occur at medium intervals.

The preceding summary relates to the curve of greatest pleasantness only. The relation of the maximal point in the curve of pleasantness to complementarism is shown in the following table:

TABLE XIII

Colour.	Complementary.	Maximal Point of the Curve for Pleasant Combinations.
1	Between 14 and 15	10
2	" 14 and 15	10
3	" 14 and 15	11
4	15	10
5	" 15 and 16	22 (11)
6	" 15 and 16	17 (21 and 1)
7	" 15 and 16	17
8	" 15 and 16	18 (21 and 1)
9	" 16 and 17	18
10	21	2
11	" 21 and 22	3
12	" 22 and 23	8
13	" 22 and 23	2
14	" 23 and 24	9
15	4	9
16	" 8 and 9	8
17	" 9 and 10	8
18	" 9 and 10	8
19	" 9 and 10	7 (9)
20	" 9 and 10	8
21	10	9
22	" 11 and 12	8 and 10
23	" 13 and 14	8 and 10
24	" 14 and 15	10

The following facts are clearly shown in the above table:

(1) In one case only (that of Curve XVI) does the maximal point of the curve of pleasant combinations coincide with the complementary colour, the latter being between 8 and 9 (nearer to 8), and the maximum at 8.

(2) All the other colours could be divided into two classes. The one class, to which those from 6 to 9, and those from 17 to 27 belong, have their maximal points near the complementary colour, but decidedly not coincident with it. To the other class belong the colours from 22 to 4, and from 11 to 15. These have their maximal point at a considerable distance from the complementary.

(3) In most cases in which the maximum and the complementary are decidedly at variance, the latter falls very conspicuously in a marked depression.

(4) It can be recognized distinctly that there is system in the shifting of the maximal point in the two groups which belong to the first class (those which have the maximal point near the complementary), and which comprise the yellow and the blue regions of the colour circle. The maximal point is always on that side of the complementary which is in the direction of the 'warm' colours (red, etc.) The same may be said of the other class, that is, the class which comprises the green and the red-purple part of the colour circle. The maximal points are for both colours on that side of the complementary which is towards the orange.

These results are very instructive in their bearing on former theories, since they practically refute every theory advanced on the æsthetics of colour combinations with the exceptions of parts of the views of Von Bézold, Brücke, Wundt and Kirschmann. It will be remembered that Von Bézold stated two propositions with regard to to colour combinations,[1] (1) that the most pleasant combinations do not occur at but near the complementary, which is substantially supported in our results, and (2) that agreeable combinations do not occur at medium intervals. This proposition is decidedly refuted.

Concerning the pleasantness of combinations of closely related colours our results, naturally, do not give very valuable information, since such combinations mostly require differences in saturation also, which we had, so far as possible, excluded from our experiments. (See No. 4 of the provisional rules for colour combinations, p. 39, *supra*.) However, that the combination of very similar colours is not necessarily unpleasant, is brought out by the abrupt rise which many of our 'pleasant' curves show at the ends. Thus our results do not contradict the views on this point of Brücke and Wundt.

Kirschmann's theory,[2] while not explicit with regard to medium intervals, does state quite clearly that the most agreeable combinations do not occur with complementary colours, but rather in the vicinity of these. This is undoubtedly true; the only addition necessary is that the most agreeable combinations need not be even near the complementary, and Kirschmann's theory in no way implies that they must.

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[1] See p. 37.

[2] See p. 38.

Summary

1. The traditional view regarding the preference of complementaries in colour combinations is unfounded. The correctness of this view is not warranted by previous experimental research, because the differentiation of the colour qualities used was not sufficient to decide whether the observers would prefer the complementary colour or some of its neighbours. There is also a lack of exact determination of the pairs which operate as complementaries in previous research.

2. With minor exceptions our results tend to confirm the view that the most pleasant combinations are not between complementary colours, but between colours of less difference in quality.

3. Our results show a slight but decided preference for the 'warm' side of the manifoldness of colour. The 'centre of gravity' of the combinations seems always to be not in the middle point of the colour circle but somewhere towards the side of the purple, red, orange and yellow.

4. The alleged aversion to yellow is decidedly contradicted by our results, at least so far as concerns dual combinations. On the contrary, in our results the yellows have, as components, the highest record, and the combinations between yellow and purple rank foremost in the list.

5. The foregoing conclusions refer to highly and equally saturated colours only. They cannot, therefore, be employed directly for the determination of agreeable colour combinations for practical purposes, for in these we have always to deal with surfaces which differ not only in colour-tone but also in saturation, a fact which may essentially change the above relations. The influence of the saturation on colour combinations remains a subject for a further investigation which has already been set on foot in the psychological laboratory of the University of Toronto.

IV

ANTICIPATORY REMARKS TO FURTHER PUBLICATIONS

In view of the importance of the relative proportions of the surfaces in judgments of colour combinations, it was thought desirable to conduct a series of experiments in which it was possible to vary the size of the surfaces. This investigation, the results of which will form the subject of the next article, was carried on during the academic year 1898-99 and continued throughout 1899-1900. The apparatus and method employed were entirely different from those used in the former experiments. The apparatus was that described by Mr. W. B. Lane in an article published in No. 1 of the Psychological Series of the University of Toronto Studies, entitled " Space-Threshold of Colours, and its Dependence on Colour Contrast." It will be remembered that Professor Kirschmann in his preliminary rules emphasizes the influence of shape, location, etc., in colour combinations. The difference produced by varying the relative proportions of the components of a colour combination seemed to be a revelation to observers. One, in his first surprise, remarked "Now, I know why one person speaks of a certain combination as pleasant while another calls it unpleasant. One is thinking of one comparative proportion, the other of another." A second observer said " All these combinations are tolerable if you get them in the right proportions."

The aim of these experiments had originally nothing to do with the problem which we treat in this article, for they deal with the influence of the relative size, but it was found later that they threw considerable light on our present problem. They form, therefore, a very desirable supplement to the foregoing experiments, since they were performed with colours spectrally very pure, and, further, all differences of light intensity were excluded. The results of these experiments will be set forth in the next article, but we may anticipate here the fact that so far as it is possible to compare these new results with those of the present investigation the legitimacy of our conclusions is essentially corroborated.

EXPERIMENTS WITH SCHOOL CHILDREN ON COLOUR COMBINATIONS

By

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EXPERIMENTS WITH SCHOOL CHILDREN ON COLOUR COMBINATIONS.

Some years ago a statistical inquiry into the colour sense of school children was conducted by the Psychological Laboratory of the University of Toronto. One of the tasks assigned to the children was to arrange six so-called principal colours, representing the colour tones of the spectrum, in a series with given colour tones at the ends, or in a circle. The intention was to find out whether children, unbiassed by any knowledge of the order of the colours according to their wave-lengths or refrangibility, would prefer, as Wundt[1] and others assert, the spectral order to any other arrangement. But the results are of some interest also from an æsthetic standpoint, and to show this is the purpose of the following pages.

The colours used were red, orange, yellow, green, blue, purple (the latter a little more towards the red than the spectral violet). The coloured objects to be arranged consisted of pigment papers pasted on thin cardboard, and punched out in discs three-quarters of an inch in diameter. Each disc was marked on the back with a letter and a number, the double marking being resorted to in order to prevent mistakes in noting down the results. The letters had no relation to the names of the colours, and the numbers no reference to the spectral order, so that if the child should happen to see the back of the discs, he should have no clue with regard to a desired order. For the sake of simplicity each of the objects will be designated in this article by the initial letter of the name of its colour.

The children performing the experiment were given each a set of the discs and the following instructions:—

1. Arrange the six colours in a line, with Y and G at the ends.
2. " " " " R and G " "
3. " " " " R and O " "
4. " " " " P and B " "
5. " " " " G and B " "
6. " " " " O and Y " "
7. " " " " B and Y " "
8. " " " " R and P " "
9. Arrange the six colours in a circle.

[1]Wundt, *Die Empfindung des Lichtes und der Farben.* (Philos. Studien, vol. iv, p. 345, *et seq.*)

The arrangements made by each child were to be those most pleasing to him (not, as in the experiment of Wundt, according to their similarity). No two were even allowed to work together, nor was outside influence in any case permitted. The grouping was thus, in every case, the result of the individual's unaided and unbiassed choice. The name and age of the child making the arrangements, the average age of the class in which he was, and some remarks concerning his eyesight were also kept for future reference.

The arrangements were made by seventy-five boys and girls ranging in age from nine to seventeen years. As a result 675 arrangements were secured. Only those children were allowed to make observations who had no knowledge whatever of the spectral order of colours, and who were unable to remember the order of colours in the rainbow. Nearly all of those who made the observations had never seen, or even heard of the spectrum, and had never paid any particular attention to the rainbow. It must be noticed also, that the correct spectral order, with R and P at the ends, is the last of the above list of eight.

In making calculations in connection with the experiment it was necessary first to determine the number of permutations of two, three, four, five or six colours that could possibly occur. These numbers are given in the table below.

TABLE

	Number of possible combinations of									
	2Colours		3 Colours		4 Colours		5 Colours		6 Colours	
	Total.	In Spectral Order.	Total.	In Spectral Order.	Total.	In Spectral Order.	Total.	In Spectral Order.	Total.	In Spectral Order.
Arranged in a straight line—										
with Y and G at ends ..	20	8	48	6	72	4	48	2	24	1
" R " G " ..	20	8	48	4	72	0	48	0	24	0
" R " O " ..	20	8	48	6	72	4	48	2	24	1
" P " B " ..	20	8	48	6	72	4	48	2	24	1
" G " B " ..	20	8	48	6	72	4	48	2	24	1
" O " Y " ..	20	8	48	6	72	4	48	2	24	1
" B " Y " ..	20	8	48	4	72	2	48	0	24	0
" R " P " ..	20	8	48	6	72	4	48	2	24	1
Arranged in a circle............	30	12	120	12	360	12	720	12	720	12

In six of the above series, with the arrangement in a straight line, the colours at the ends are neighbours in the colour circle. The other two series (R and G, B and Y at the ends) were added as a test as to whether the juxtaposition of neighbouring colours of the spectrum receive any preference, even in cases where complete spectral order is impossible.

The results of the experiments may be utilized, besides their original purpose with regard to the spectral order, as a test for the much disputed question whether in colour combinations small intervals, medium intervals or maximal intervals have the most pleasant effect. It is true that the above trials deal always with combinations of six colours. Thus these experiments can only give, strictly speaking, data about the æsthetic value of combinations of two, three, four, etc., colours when forming part of combinations of six.

The following is a list of the possible combinations, and the number of times they were chosen.

I. *Yellow and Green at the ends.*

Combinations.	Number of times chosen.
Y O R P B G	16*
Y O R B P G	10(*)
Y O P B R G	2
Y O P R B G	1
Y O B R P G	3
Y O B P R G	3
Y R P B O G	1
Y R P O B G	2
Y R B O P G	3
Y R B P O G	1
Y R O B P G	0
Y R O P B G	4
Y P B O R G	3
Y P B R O G	2
Y P O R B G	0
Y P O B R G	6†
Y P R O B G	2
Y P R B O G	2
Y B O R P G	1
Y B O P R G	5(†)
Y B R P O G	3
Y B R O P G	1
Y B P R O G	1
Y B P O R G	1

II. *Red and Green at the ends.*

Combinations.	Number of times chosen.
R O Y P B G	15(*)
R O Y B P G	5
R O P B Y G	4
R O P Y B G	2
R O B Y P G	0
R O B P Y G	3
R Y P B O G	1
R Y P O B G	4
R Y B O P G	6(†)
R Y B P O G	1
R Y O B P G	2
R Y O P B G	4
R P B O Y G	1
R P B Y O G	0
R P O Y B G	2
R P O B Y G	1
R P Y O B G	1
R P Y B O G	1
R B O Y P G	0
R B O Y P G	8(†)
R B Y P O G	5(†)
R B Y O P G	4
R B P Y O G	0
R B P O Y G	1

III. *Red and Orange at the ends.*

Combinations.	Number of times chosen.
R G Y P B O	2
R G Y B P O	1
R G P B Y O	2
R G P Y B O	5†
R G B Y P O	3
R G B P Y O	2
R Y P B G O	5
R Y P G B O	3
R Y B G P O	1
R Y B P G O	3
R Y G B P O	4
R Y G P B O	4
R P B G Y O	15*
R P B Y G O	3
R P G B Y O	1
R P G Y B O	0
R P Y G B O	2
R P Y B G O	0
R B G Y P O	0
R B G P Y O	1
R B Y P G O	7(†)
R B Y G P O	5
R B P Y G O	3
R B P G Y O	1

IV. *Purple and Blue at the ends.*

Combinations.	Number of times chosen.
P G Y R O B	0
P G Y O R B	5
P G R O Y B	1
P G R Y O B	3
P G O Y R B	3
P G O R Y B	2
P Y R O G B	2
P Y R G O B	5†
P Y O G R B	3
P Y O R G B	0
P Y G O R B	3
P Y G R O B	0
P R O G Y B	7(*)
P R O Y G B	15*
P R G O Y B	1
P R G Y O B	1
P R Y G O B	7
P R Y O G B	14(*)
P O G Y R B	1
P O G R Y B	0
P O Y R G B	1
P O Y G R B	3
P O R Y G B	1
P O R G Y B	1

V. *Orange and Yellow at the ends.*

Combinations.	Number of times chosen.
O P B R G Y	0
O P B G R Y	2
O P R G B Y	1
O P R B G Y	2
O P G B R Y	2
O P G R B Y	8(†)
O B R G P Y	5†
O B R P G Y	4
O B G P R Y	1
O B G R P Y	1
O B P G R Y	1
O B P R G Y	8
O R G P B Y	0
O R G B P Y	4
O R P G B Y	5
O R P B G Y	17*
O R B P G Y	5
O R B G P Y	0
O G P B R Y	3
O G P R B Y	0
O G B R P Y	0
O G B P R Y	5
O G R B P Y	1
O G R P B Y	4

VI. *Green and Blue at the ends.*

Combinations.	Number of times chosen.
G P Y R O B	1
G P Y O R B	1
G P R O Y B	2
G P R Y O B	2
G P O Y R B	0
G P O R Y B	2
G Y R O P B	1
G Y R P O B	3
G Y O P R B	4
G Y O R P B	18*
G Y P O R B	0
G Y P R O B	3
G R O P Y B	2
G R O Y P B	4
G R P O Y B	4
G R P Y O B	2
G R Y P O B	11†
G R Y O P B	3
G O P Y R B	2
G O P R Y B	3
G O Y R P B	3
G O Y P R B	1
G O R Y P B	1
G O R P Y B	1

VII. *Blue and Yellow at the ends.*

Combinations.	Number of times chosen.
B P O R G Y	5
B P O G R Y	0
B P R G O Y	2
B P R O G Y	11(*)
B P G O R Y	2
B P G R O Y	4
B O R G P Y	5†
B O R P G Y	1
B O G P R Y	1
B O G R P Y	4
B O P G R Y	7
B O P R G Y	0
B R G P O Y	0
B R G O P Y	4
B R P G O Y	1
B R P O G Y	7
B R O P G Y	1
B R O G P Y	4
B G P O R Y	1
B G P R O Y	3
B G O R P Y	1
B G O P R Y	1
B G R O P Y	3
B G R P O Y	3

VIII. *Red and Purple at the ends.*

Combinations.	Number of times chosen.
R O Y G B P	20*
R O Y B G P	1
R O G B Y P	1
R O G Y B P	5
R O B Y G P	1
R O B G Y P	0
R Y G B O P	1
R Y G O B P	1
R Y B O G P	4
R Y B G O P	3
R Y O B G P	1
R Y O G B P	0
R G B O Y P	5
R G B Y O P	5
R G O Y B P	0
R G O B Y P	8†
R G Y O B P	0
R G Y B O P	1
R B O Y G P	2
R B O G Y P	5
R B Y G O P	2
R B Y O G P	2
R B G Y O P	2
R B G O Y P	3

IX. *Arranged in a circle.*

The 720 possible permutations cannot be given. The table is confined to those actually chosen.[1]

Combination.	Number of times chosen.	Combination.	Number of times chosen.
Y O B R P G	1	Y P O R B G	1
Y P O G R B	2	Y P O G B R	1
Y G R O P B	1	Y P B G O R	1
Y O R P B G	6*	Y O B G R P	1
Y O R B P G	2	Y B G R O P	1
Y O P B G R	2	Y O P G R B	3
Y B P O G R	2	Y B R P O G	1
Y B O P G R	1	Y R B G P O	1
Y G P B R O	1	Y B R G P O	1
Y G B R O P	1	Y B O R P G	1
Y G B P R O	6*	Y G R O B P	2
Y R O G P B	1	Y B G R P O	2
Y G B O P R	2	Y B G O P R	1
Y P G R B O	2†	Y G B R P O	1
Y B P R O G	1	Y B R G O P	2
Y B O G R P	2	Y O G B P R	1
Y G O P R B	3	Y G B R P O	1
Y B O P R G	2	Y G B O R P	1
Y G O R P B	1	Y R O B G P	1
Y R G P O B	1	Y P B R O G	1
Y R G B O P	1	Y P G B O R	1
Y B O R G P	1	Y O P R G B	1
Y P R B O G	2	Y P R B G O	1
Y B G P R O	2	Y P O B G R	1†

[1] The cutting of the circle, for the sake of designation, in this table between yellow and orange is perfectly arbitrary but irrelevant.

In the above tables the combinations with the least possible difference between neighbouring colours, *i.e.* the arrangements in spectral order, are marked by an asterisk, and close approach to this order by an asterisk in brackets. Combinations with maximum difference between the members are marked by an obelus, and sometimes close approach to such order by an obelus in brackets.

A glance over these tables reveals the fact at once that the combination with minimum difference (spectral order) has a decided preference. Whilst all the other combinations, with a few exceptions, are chosen fewer times than ten, the arrangement in spectral order is never below fifteen and even reaches eighteen and twenty. That is to say, the spectral order has been preferred throughout in twenty to twenty-five per cent. of all cases. In the two series with red and green, and yellow and blue at the ends, complete spectral order is impossible, yet we see that the nearest possible approach to spectral order is most favoured. But also in the other series, a combination which is a close approximation to the spectral order, with minimum differences, is sometimes given a number as high as and even above ten. It is possible that in many instances where the children selected such close approximations, impatience or fatigue prevented a still more favourable result.

It will further be noticed that combinations with the maximum difference between the members, *i.e.* arrangements with juxtaposition of contrasting colours, seem to be far less pleasing than the spectral order, although they are more often chosen than many other combinations. It is remarkable that certain approximations to the contrasting combinations are more favoured than those combinations themselves.

The actual results may now be compared with the numbers, which, according to the theory of probability, should fall to the share of the combinations with minimum differences.

A. Arrangement in a Straight Line.

I. *Combinations of Two Colours.*—The number of combinations with minimum differences (spectral order) is to the number of all possible combinations as 8 to 20. Since there are in each series five combinations of two, and there were eight series, each performed by seventy-five children, there were 3,000 combinations of two chosen. According to the tables, combinations of two neighbouring colours occurred 1,347 times. According to the law of probability there should be only 1,200 cases of such combinations. Thus, combinations of neighbouring colours amount to 45 per cent. instead of 40 per cent., in spite of the fact that not all these combinations are favourable to a tendency for the arrangement in spectral order. For instance, in an arrangement with yellow and green at the ends, the combination orange-red will correspond to the spectral order, whilst red-orange, in itself just as good a combination of neighbouring colours, will be decidedly adverse to this order. It must be further noticed that the number of 3,000 combinations fixed upon, as stated above, is a little too high, for a few observations had to be omitted on account of incompleteness or inaccurate recording. It was impossible in some cases, owing to the death of the observer or other difficulties, to have the experiment repeated. Thus the results are actually a little more favourable to the combination with minimum differences than they appear in the numbers stated above. The same must be said for the other combinations.

II. *Combinations of Three Colours.*—For three colours the number of combinations with minimum difference is to the number of all possible combinations as 6 is to 48, in the case of two neighbouring colours being at the ends of the series, and as 4 is to 48, in the two series with complementaries at the ends. There are altogether 1,800 ($75\times6\times4$) combinations of three chosen in the series with neighbouring colours at the ends. In 532 of these cases the combination according to the spectral order was preferred. According to the law of probability, only 225 favourable cases might be expected. Thus instead of $12\frac{1}{2}$ per cent., we find almost 30 per cent. In the two odd series where there are 600 possible combinations of three ($2\times4\times75$), the ratio between favourable and all cases should be as 4 is to 48, but instead of $8\frac{1}{3}$ per cent. of all cases we find almost 13 per cent., *viz.*, 78 cases favourable to the spectral order.

III. *Combinations of Four Colours.*—We shall now only refer to the series with neighbouring colours at the ends. Since each child fixed upon 6×3 combinations of four, there are $75\times18=1{,}350$ combinations selected. Only four of every 72 are combinations with minimum difference. But this order was preferred in 322 cases, that is, in almost 24 per cent. of all actual cases, against $5\frac{5}{9}$ per cent. as according to the law of probability.

IV. *Combinations of Five and Six Colours.*—There were selected by the children $6\times2\times75=900$ combinations of five colours. The number of combinations with minimum difference is to the total number of all possible combinations as 2 is to 48, or $4\frac{1}{6}$ per cent. But we find that in 202 selections the spectral order was preferred, which amounts to $22\frac{4}{9}$ per cent. of all actual cases. This same number is valid also for the combinations of six colours, which are, for arrangements in which the ends are fixed, identical with those of five. We have here 101 favourable cases out of 450, or $22\frac{4}{9}$ per cent.

B. Circular Arrangement.

The actual frequency of the selections favourable to the spectral order, and what should be expected according to the law of probability in case of the arrangement in a circle, may be seen from the following table which is self-explanatory.

	Number of Selections Actually Made.	Number of Selections in Spectral Order.	Percentage of the Selections in Spectral Order.	Percentage expected according to the Law of Probability	Ratio between favourable comb. and all comb. acc. to the Law of Probability.
Combination of 2 Colours..	450	211	$46\frac{2}{3}$	40	180:450
" 3 "	450	96	$21\frac{1}{3}$	10	45:450
" 4 "	450	79	$17\frac{5}{9}$	$3\frac{1}{3}$	15:450
" 5 "	450	72	16	$1\frac{2}{3}$	$7\frac{1}{2}$:450
" 6 "	450	72	16	$1\frac{2}{3}$	$7\frac{1}{2}$:450

In conclusion, these experiments seem to establish beyond doubt that in multiple combinations of colours, the juxtaposition of similar colours (small intervals) is decidedly preferred to that of complementary colours (maximal intervals), and also to that of colours of medium intervals.